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DIVERSE IDEALS AND DIVERGENT CONCLUSIONS IN THE STUDY OF BEHAVIOR IN LOWER ORGANISMS[1]

H. S. JENNINGS

The living interest of the study of the behavior of animals lies in the concrete facts: in what the animals *do*. A kinematograph would perhaps make the best possible presentation of this subject. But to-day I am going to deny myself the pleasure of presenting the concrete facts, and deal rather, since this is a conference on research, with certain problems of investigation, in the behavior of the lowest organisms. I wish to try to present the main general results of investigation in this study of behavior at its lowest terms, together with its relation to certain general theories; to show why there is such marked disagreement in the accounts given by different investigators; and to point out the fundamental problems for future work. I realize only too strongly that I am not a psychologist, and that my claim to a hearing before psychologists lies only in that my work has been with matters which the psychologist needs to take into consideration. I shall therefore not deal with matters that are psychological in the subjective sense, but with some of the biological relations that underlie psychology.

In recent years a new spirit, a new desire, has permeated biological science in every division,—in brief, the desire *to see the processes of nature occurring*, and to modify and control these processes—not merely to judge what processes *must*

[1] Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

have occurred.[1] In the words of the young Clerk Maxwell[2] we wish to see the "particular go" of the processes of nature. This is the essential point in the present wide use of the experimental method in biology.

Contrasted with this is an earlier method of work, which may be expressed as follows: Certain conditions were seen to exist. From this, conclusions were drawn as to what *must have occurred*, in order that these conditions might exist. If we succeeded in imagining a process that would satisfactorily account for what exists,—then that was a sufficient explanation. If we found further that this explanation fitted certain other facts, which it was not devised to explain, then it was felt that the explanation was confirmed; was *verified*, even though the supposed process had not itself been observed.

This method of interpretation was long the common one throughout the general biological sciences, zoölogy and botany, and was much in use even in physiology and psychology. The great example is the theory of evolution, together with the special theories grouped about it, concerning the origin of particular organisms, or of particular structures and functions.

In the new spirit of work, the desire is to see things happening, not to conclude what must have happened. If evolution occurs, we wish to *see* it occurring; if acquired characters are inherited, we want to see a few acquired and inherited. We wish to see the processes themselves, not merely the result of supposed processes.

This is the spirit that has led to the recent rebellions against accepted doctrines. Its more thorough-going partisans reject all explanations that are merely devised to explain results. If they can't see evolution occurring, they conclude that it does not occur. They refuse to accept the principle of selection because they do not see it at work. They tell us, with von Uexküll,[3] that Darwinism is to be stricken from the list of scientific theories; with Driesch,[4] that Darwinism fails all along the line. Any explanation that deals with processes not observed is ruled out. The ideal is to build up an account of nature that shall consist entirely of statements that can be verified; that is, in which any process said to occur can be *observed* to occur, if the conditions are properly supplied.

[1] Compare the statement of the object of scientific investigation by Loeb, to whom perhaps more than to any other person the prevalence of this ideal is due. (Loeb, J., Preface to *Studies in General Physiology*, 1905.)

[2] As quoted by James, *Pragmatism*, p. 197.

[3] Die neuen Fragen in der experimentellen Biologie. Rivista di Scienza "Scientia," 1908, vol. 4.

[4] Science and Philosophy of the Organism, vol. 1, p. 269.

It is only by grasping thoroughly this ideal that one can understand the basis for the views set forth by its exponents; the startling conclusions drawn by such men as Driesch, v. Uexküll and Loeb.

The same spirit and method have been carried into comparative psychology, and the first result was to banish that which had heen supposed to constitute the very subject under examination. "Comparative psychology" became "animal behavior," for psychological processes cannot be observed in animals, and are therefore ruled out. Their very existence was called in question, and all discussion of them was denounced as idle speculation; comparative psychology was denied a place in science.[1]

This spirit has doubtless led to iconoclastic excesses in its zealous partisans; there has been a tendency to forget that within the next ten thousand years many processes may be observed that we have not succeeded in finding in the decade or two in which this spirit has ruled, so that sweeping negative conclusions are hardly warranted. But as a basis for a positive working method the ideal is sound and valuable. We must observe the processes of nature, not merely guess at them. Certain results and problems arising from the study, in this spirit, of the behavior of lower organisms, is what I wish to bring before you. Let us accept the tests imposed by this ideal, and see what we have reached.

Strangely, perhaps, this spirit of work has not succeeded in bringing about agreement in the study of behavior in the lower organisms. As you know, extreme divergences on what seem fundamental matters are found between the accounts given by different sets of experimental investigators in this field. Certain investigators show the phenomena as simple and dominated by easily understood mechanical factors; concrete physico-chemical explanations are presented as throughout satisfactory. The whole matter is set forth in such a way that he who runs may read and understand. Here, one feels, is a field that has been set in order; here the physico-chemical methods of attack have shown themselves adequate. And what a contrast to the results of older methods of study! No psychological discussions, no anthropomorphism, no teleology; no uncertainty, but simplicity, uniformity, constancy everywhere; simple mechanical considerations suffice for all. Truly a triumph of the mechanistic views of life phenomena!

But to the student's perplexity, he finds another group of

[1] See for example, v. Uexküll, *Im Kampf um die Tierseele*, reprinted from Asher and Spiro's Ergebnisse der Physiologie, Jhrg. 1, 1902, and Nuel, *La Psychologie Comparée est-elle Légitime?* Archives de Psychologie, T. 5, 1906, pp. 326-343.

experimentalists, working with equal thoroughness, that give a very different account of these matters. They tell us that behavior in the lower organisms is extremely complex, varied and variable. The simple mechanical explanations are largely rejected as unverifiable and inadequate to the facts. The regulatory character of behavior, which has given rise to teleological and vitalistic doctrines, is insisted upon. Resemblances are pointed out between the behavior of lower organisms, and that of higher animals and man. The striking differences between the behavior of even the lowest organisms, and that of inorganic bodies is set forth.

The members of the first group of investigators thereupon accuse those of the second group of most grievous sins. They accuse them of indeterminism, of anthropomorphism, of "psychologizing," of teleology or finalism; of vitalism.[1] The members of the second group plead not guilty to these charges, and are inclined to respond by characterizing the work of the first group as superficial, misleading and generally inadequate.

Why this divergence? Why should investigators working on the same field under the same principles fail in this radical way to come to agreement?

The divergence appears to me largely due to certain differences in the plan and method of investigation; to difference in opinion as to what knowledge concerning behavior is of worth. There are two diverse ways of attacking the problems of behavior in lower organisms, and these two ways lead naturally to the two divergent sets of views that we have sketched.

1. The first method, and the one giving the brilliantly simple results, may be called the method of the physico-chemical key, or from certain points of view the *synthetic* method. It consists most typically in taking some single physico-chemical principle, or the action of some simple physical agent, and using this as a key to unlock the secrets of behavior. One takes osmotic pressure, or surface tension, or the electric properties of ions, and traces its operation throughout behavior; or he studies the direct action of gravitation, of heat, of light, on the movements of animals. That is, one keeps his eyes on this physico-chemical principle or agent; this, not the organism, forms the unit of work, the object of investigation. Experimentation consists in subjecting organisms to the operation of the principle or agent in question. When we have thus traced the action of the various physico-chemical principles or agents with which we are acquainted, we have the science of behavior; this science is thus built up synthetically from the simplest elements.

We have all seen this method applied, with brilliant results,

[1] For a compendium of such accusations, the recent book of Dr. Bohn may be consulted (*La Naissance de l'Intelligence*, 1909). See also Loeb, *Journ. Exper. Zool.*, vol. 4, 1907, pp. 151-156.

in many fields of biology. It has given us in behavior, among other things, the famous tropisms,—the direct, uniform effect of constantly acting physical agents on living organisms.

Those who work in this way commonly hold that it is the only method of work that is worth while. The purpose of investigation is held to be, to work out the physics and chemistry of biological phenomena. The way to do this is evidently to take our known physical and chemical laws, and trace their operation in the biological field. A fundamental principle for this method of work is this: physico-chemical action is constant; it can be depended on. With the same reagent acting on the same material, we must get always the same results. The organism is the material; the reagents to be used are the known chemical and physical ones; the results form our science of behavior.

This is the method of investigation which gives the plain and simple results. The ogres of anthropomorphism, of teleology, of vitalism, that have devoured so many biologists, nowhere so much as show their heads to the traveller upon this route.

2. The second method of study may perhaps be called the analytical method. It is based upon interest in the organism rather than on interest in physics and chemistry, and it makes the organism the unit of work, the object of investigation. The investigator wishes to know all about a given organism. Among other things, he wishes to know its entire behavior, and incidentally whether its behavior is as a matter of fact fully accounted for by known physico-chemical principles, and if not, how much and what is left over.

The actual method of work is to first watch the organism under its natural environment, till one finds out all things it does. Then the environment is changed a little, to see what difference this makes in the behavior. We thus try all sorts of different ways of getting the animal to change its behavior,—including the application of definite chemical and physical reagents of most varied character. We find all the different things the animal can do, and we work out the determining causes and conditions for each. We find all the different ways in which the animal may react to the same stimulus, and we find all the different methods of causing it to give each reaction. We thus find the organism's system of behavior and the things that influence it,—becoming acquainted with the creature as we might get acquainted with a person with whom we are thrown much in contact.[1] We carry on the same sort of work with different organisms, and compare them.

[1] Compare v. Uexküll's statement that the first requirement is "Die andauernde und eingehende Beobachtung des lebenden Tieres in seinem Milieu" (*Leitfaden in das Studium der Experimentellen Biologie der Wassertiere*, Wiesbaden, 1905, p. 75.)

Thus in this method we begin with the complex organism and attempt to analyze its behavior, proceeding to simpler and simpler determining factors, till we get the simplest that can be reached. In this way we must of course finally reach the simple physical and chemical factors with which the investigators by the other method start, provided that the latter are real factors. But since the analysis is difficult and slow, for a long time we are forced to deal with complex components; we separate the original complex mass into smaller and smaller complexes, each to be thoroughly analyzed later. These complex components are not elementary physical or chemical factors. We deal with organisms as wholes, and with such concepts as respiration; the securing of food; protection and defense; care of the young, construction of nests and the like; things which cannot now be expressed in the terminology of physics and chemistry.

Thus the investigator by this analytical method does not agree that work with the elementary chemical and physical factors is the only thing worth while; he finds, in contrast to the worker by the synthetic method, that the relations between these complex, biological components are in themselves decidedly worth while; that they are indeed of the greatest interest and importance. Such are the relations of organisms to each other; the preying of one on another; the conjugation of organisms; the selection of food, and other relations of behavior to metabolism; the relation of behavior to defence and protection, the similarities and differences between the behavior of different organisms, and the like. These things are not physico-chemical concepts, and the relations between them fall outside the field of view of the investigator who, in the synthetic method, works only with such concepts.

These biological interrelations form then a large part of the field of study by the analytical method. But it is of course clear that there is nothing incompatible in all this with further analysis into elementary physical and chemical factors. We merely find it impracticable and useless to wait until such analysis is complete before dealing with the important biological interrelations. Indeed, we should have to deal equally with these biological relations even if the analysis into physico-chemical factors were completed; they would be quite lost sight of if we limited ourselves to an account of the simple chemical and physical factors. The interrelations of mountain, stream and forest are important, but they are not easily set forth in terms of the movements of electrons or ions; and the working of a Hoe printing press is an equally refractory subject for presentation in that way. As Ritter has well emphasized in a recent paper "you can never give a full account of any whole in terms of its elements."[1]

[1] Ritter, W. E.: *Life from the Biologist's Standpoint.* Pop. Sci. Monthly, *75*, p. 177.

But of course the work of analysis is not finished, in the case of the organism, as in the landscape or the machine, until each part is resolved into the elementary determining factors with which the synthetic method starts.

Thus the synthetic method starts at the bottom and attempts to see how the complex behavior is built up from the simplest elements, while the other method starts with the given complex,—the organism and its behavior,—and attempts to resolve this into its elements. It is of course never possible to classify men absolutely, but typical examples of the different sorts of investigators can be given. For the synthetic method, beginning with the simple chemical and physical principles, the great example is of course furnished by Loeb and his followers. For an example of the investigators on lower animals that begin with the complex and attempt to analyze it, we may name von Uexküll. We may cite also the brilliant work done on behavior in lower organisms under Professor Hodge's direction, in this university. The present speaker has likewise followed this analytical method; and it has almost of necessity been the method employed by workers on higher animals.

Now, unless we are mistaken in our fundamental premise that physico-chemical explanations can be given for behavior, both these methods of work are quite justified, and they must finally, when they have finished their work and covered the ground thoroughly, come to the same results. But one method begins at one end, the other at the other end, and since "science moves but slowly, slowly," we find that at any given time the two have not met; they have not covered the same ground; they do not see the field alike,—and there arise misunderstandings and controversies.

The first method of exploring behavior may be compared to the method of the prospector who goes into a new country seeking gold. His eyes are all for the signs of gold; he may find it and come back laden, as Loeb has done again and again. But he is not so likely to bring with him a correct map of the country as is the surveyor.

The second method of exploring behavior may be likened to that of the geological survey. The first business is to make a topographical map of the country, so that we may guide ourselves and get a general view. The next is to make a systematic and detailed examination of all regions, to find out the relation of river and mountain and forest; of formations, strata and soils.

The surveying party is not so likely as the prospector to return from a given trip with gold, but it is naturally more likely to have made an accurate survey of the country, and to

have determined the various things that are there to be found. It may even be able to correct certain distorted views reported by the prospector. The investigator who works in this way is likely to give a more adequate 'natural history' of behavior than the worker by the other method. He will have a more complete idea of the matters remaining to be worked out, the difficulties to be met, and the relation of the different parts to each other.

It is the difference in the pictures presented by the men working in these two diverse ways that is the main cause of the controversies that have arisen. In other fields of biological investigation we find the same two methods of work, and there appears to be an almost "irrepressible conflict" between their representatives, though the divergences in results are perhaps less crying than in behavior. My present purpose is to try to compare in certain general features the picture presented by the two different sets of workers, in the behavior of lower organisms, pointing out the ground for the divergences.

In the unicellular organisms we have theoretically the simplest condition of affairs that we can find; we have the problem of behavior at its lowest terms. What are the characteristics of this behavior? What bearing have the phenomena here on the possibility of giving physico-chemical explanations for natural phenomena? What relation have they to those three reprobated tendencies or doctrines of which we hear so much of late, in attack and defence, in accusation and recrimination: to vitalism, to anthropomorphism, to teleology or finalism? Why do these doctrines maintain themselves, and how far is there justification for them?

Now, it is evident that the first or synthetic method of study, from its very nature, avoids any phenomena on which these reprobated doctrines could possibly be based, any phenomena not manifestly of a physico-chemical character. It simply does not look at them. It devotes itself to visible physical and chemical matters, leaving the rest out of the field of view. This is done deliberately and intentionally, because it does not consider other matters ripe for treatment. The point of view of such investigators is well expressed by Bohn, in his recent eulogy of Loeb: "as a man of positive science, he does not waste his time reasoning about things that have resisted scientific analysis."[1] I believe that no criticism can be made of this procedure, as a method of work, provided it does not represent as non-existent the parts with which it does not deal. Its chief danger is that it tends to make one forget how much remains

[1] "—En homme positif, il ne perd pas son temps à raisonner sur des choses qui ont résisté à l'analyse scientifique." Bohn, *La Naissance de l'Intelligence*, 1909, p. 44.

to be done. To point out the limits of our present knowledge and the problems that remain to be solved is almost as important as to set forth what has already been worked out. And this is particularly true in fields where that which is understood is but a fraction of that which exists. The investigator indeed must keep his eye on what remains to be done, rather than on what has been done.

Thus the freedom from anything vague or uncertain; complex or difficult to understand; anything not resembling common physico-chemical action; anything that could suggest anthropomorphism or finalism or vitalism,—that we find so striking in the accounts of investigators working by the synthetic method, is after all *a priori*, and due simply to the omission of everything of that sort. The reader, finding in these accounts nothing to suggest the difficulties, concludes that the difficulties do not exist; that this is a field where the resolution into physics and chemistry has become complete.

But the worker by the analytic method is unable to escape so easily from the difficulties. Making the complex organism his object of study, interested in this for its own sake, and attempting to omit nothing, he finds himself at once confronted with a mass of phenomena that have not been analyzed into physico-chemical factors; phenomena in the highest degree complex, varied and peculiar. He is even pained to find that many of the proposed physico-chemical explanations are inadequate for the very phenomena which they have been called in to explain. He is forced to deal with that storehouse of materials from which anthropomorphism, finalism and vitalism have drawn their supplies. He endeavors in a preliminary way to analyze and arrange these, and to report to those interested, what one finds when one examines thoroughly the behavior of lower organisms. If he is honest, he is compelled to report even in the lowest organisms many features that resemble features in the behavior of man. He finds inescapable relations between the present actions of the organism and certain later conditions,—resembling what is seen in the purposive action of man. Though these discoveries may be unwelcome, the surveyor with a conscience is forced to set them down on his map and mention them in his description.

And then the storm bursts upon his head. Anthropomorphism! Teleology! Finalism! Vitalism! cries in horror the aggressive physico-chemist, and the rash investigator is drummed out of the mechanistic camp. The teleologist and the theologian seize upon the account of actions resembling those we perform with a purpose, and count the author among their allies. The upholders of vitalism hold out the right hand of fellowship to

the investigator who has shown the inadequacy of physico-chemical explanations.[1]

Yet the unfortunate investigator whose sin has been to try to tell the whole truth is still a believer in the validity of physico-chemical explanations; in the necessity of formulating all processess causally; he holds that teleology cannot be substituted for causation, that to perceive the resemblance between man and lower animals does not give a causal explanation of either; he is convinced of the error of vitalism. Rejected from the camp of the physico-chemists, and refusing to take refuge with the enemy, he is left an outcast.

Where is the mistake? Do such accounts of behavior as he gives really imply that physico-chemical explanations are wrong in principle,—as both the vitalists and the aggressive physico-chemist seem to agree that they do?

If they do—alas for physico-chemical explanations—for such accounts are certainly correct! But how incredibly short-sighted; how faint-hearted, is the physico-chemist that takes such a view. These accounts of behavior neither justify finalism and vitalism as substitutes for causal explanations, nor do they refute mechanism. They show merely that there is still much work for us to do; that the end of analysis is by no means in sight; that some of the methods of analysis have been inadequate; that the problems are much more extensive and difficult than some have imagined. They have no more weight in overthrowing physico-chemical explanations than has the existence of an unexplored country, in showing the impossibility of exploration. True, the choice between physico-chemical and vitalistic formulations must for the present, for a large part of the phenomena at least, be based upon *a priori* grounds, not upon demonstration. But whoso has imagined otherwise should remember the saying of Newton, that he had but picked up a few pebbles on the shore of the ocean of knowledge, and should ask himself whether he has really gathered into his pocket that measureless ocean and all its shores. When he has done that, he may have established by demonstration the complete adequacy of physico-chemical explanations; until he has done it, we must rely upon *a priori* considerations.

If we accept from beforehand the programme of physico-chemical or mechanistic explanation, how are we to conceive the facts in the behavior of lower organisms? What are the chief facts discovered in a careful survey; what is their relation to vitalism, to anthropomorphism, to teleology?

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[1] See for example Driesch, *The Science and Philosophy of the Organism*, vol. 2, and v. Uexküll, *Die neuen Fragen in der experimentellen Biologie, Rivista di Scienza "Scientia,"* vol. 4, 1908.

1. Let us consider vitalism first. The experimental investigator is interested in vitalism on account of its practical bearing on the justifiability of his methods of work. He can experiment only by altering in some way the configuration of matter and energy: *i. e.*, by making some physical or chemical change. Can he hope to carry through this method consistently and completely, explaining all divergences in results by differences in the preceding configuration of matter and energy? Or will he find cases of divergence in results where there are no foregoing differences in the configuration of matter and energy, so that from throughout identical configurations diverse results follow? If the latter is the case, then evidently the experimental method fails.

The question here is not merely whether the explanations hitherto commonly used in physics and chemistry are entirely adequate ones. It is the deeper question, whether when adequate principles of explanation *are* used in the physics and chemistry of things not alive, other and diverse principles of explanation will be required for the study of living things. The question is as to whether there is a real, fundamental diversity between the necessary principles of explanation for living and for non-living things.

To hold that the necessary principles of explanation are not diverse in the two cases; to hold then, in opposition to vitalism, that physico-chemical principles of explanation are generally adequate, is to hold this:

What happens in any system of matter and energy at any period is determined by the configuration of matter and energy at a preceding period. Experimentally, therefore, differences in resulting conditions in any two cases will always be found preceded by differences in foregoing conditions, so that nothing happens without its determining factors in the previous configuration of matter and energy. If we search with sufficient care, we shall always be able to find in matter and energy a determining factor for everything that occurs. Two identical combinations of matter and energy cannot produce different results, nor two different combinations absolutely identical results.

This of course leaves open the question whether there may not be present in certain cases additional, subjective, properties, and also, whether it may not be proper to make, besides the physico-chemical explanations, other explanations that take into consideration these subjective properties. Furthermore, it does not deny that there are in living things combinations of matter and energy not found elsewhere, so that methods of action may occur that are not found elsewhere; to this we return later.

In attempting to understand and explain the behavior of

the lower organisms, one of the chief stumbling blocks has been a preconceived idea as to what we should find. Many have imagined these creatures to be a sort of link between organic and inorganic material. Higher animals it is generally recognized are complex structures, comparable in this respect to machines. In the lower organisms it was expected that we should find mere masses of a certain sort of material, and the study of behavior would be merely the study of the chemistry and physics of this material. This idea has perhaps been most explicitly developed by Le Dantec.[1] This *a priori* conviction, often unformulated, maintains itself most obstinately in all thought on the behavior of lower organisms, particularly of those composed of but a single cell.

The most fundamental result of the study that has been made is to show the incorrectness of this idea; to show that lower organisms, like higher ones, are typical *arrangements* of material; are structures; are in this respect machine-like; not masses of a uniform substance.

This is evident both from a study of the activities of these organisms, and from a direct examination of their structure. Such a unicellular creature as Stentor has many systems of differentiated organs, often acting independently of one another; its structure can only be characterized as inconceivably complex. Paramecium with the microscope is seen to be a most complicated machine, running at a high rate of speed. In studying the behavior of these creatures, what we find out is *how certain machines work*, rather than the direct physical and chemical properties of a certain substance.[2]

The practical difference in the results of study, due to this fact, is perhaps much greater than appears at first thought. To this difference between what was expected and what was found are due most of the paradoxes and difficulties in behavior, and most of the apparent failures of direct physico-chemical explanations. To it are due the recrudescence of anthropomorphism, finalism, vitalism. Let us try to see how these things come about. In so doing I shall have to remind you of certain facts that are sufficiently obvious, yet that have been neglected of late in work on lower organisms.

The first principle of physico-chemical explanations is that the chemical and physical properties of the living substance

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[1] *La Matière Vivante.* Paris, 1895.

[2] Whether, from other points of view, an organism is not also something more than a machine, is another question. If we mean by a machine any system in which the configuration of matter and energy determines what happens, then the contention in the text is that for purposes of causal explanation of what happens only this machine-like character requires consideration.

determine its reactions, so that from a knowledge of these properties we can predict the reactions; and further that the same reagent acting on the same substance is bound to produce the same result. Any statements to the contrary are looked upon as controverting the principle of causal determination and leading to vitalism.

But when the substance is arranged in typical ways, this principle, though essentially true, becomes practically false and misleading. From the same mass of substance we can make many different arrangements or machines, acting in entirely different ways, so that we could never predict the reactions of the machines from a knowledge of the chemical and physical properties of the unarranged substance. From a certain mass of material we could make either a clock or a doorbell or a steel trap or a musical instrument,—and we could easily so arrange these that each would respond in its characteristic way when acted upon by an electric current. We could, moreover, make the same machines, showing the same reactions, from a different kind of material, with different properties. We could then never predict the reactions of these by knowing merely the chemical and physical properties of the material of which they are composed. The specific action of each depends on the specific arrangement of its material.

This is exactly what we find in organisms, including the lowest as well as the highest. From it there result certain relations that are extremely perplexing, though they can be illustrated from inorganic combinations as well as from organic ones. Let us examine some of these.

First, both in inorganic arrangements or machines and in organisms we find that the same substance reacts to a given reagent sometimes in one way, sometimes in another. It all depends on how the material is arranged;—whether as a clock or as a doorbell; whether as a Stentor or as a Paramecium. Furthermore, by a slight shifting of the arrangement, we find that the very same piece of material is caused to react to the same reagent in entirely different ways. A typewriter responds to mechanical stimulations by printing English lower case letters. After momentary pressure of a certain lever, it responds to the same stimulations by printing capital letters, or by printing numbers or conventional signs. It would be easy to so arrange it that after a shift of a lever, it would respond with script or italics or German or Russian or Greek letters. Here we have parallel conditions to what we find in lower organisms. What we find to be true for one organism or one individual turns out not to be true for another, and even what we find to be true for a given individual will not hold later for the same

individual. The principle of the *shift* in arrangement comes into play continually, giving us most inconstant results. Yet there is no breach of determinism here; the determining feature lies both for the machine and for the organism in the arrangement of parts.

In view of these facts, it is not surprising that we often get nothing fundamental by determining the action of a given chemical or physical agent on living substance. It is often assumed, tacitly or openly, that after such a determination is made its results can be transferred to other masses of living material; can be generalized. But the effects seen are usually due to the characteristic arrangement of the material acted upon; they disappear or are reversed as soon as we work with material in other arrangements. We cannot therefore expect the study of the direct simple action of chemical and physical agents to help us greatly in understanding why an animal does what it does, though this has been heralded as the one right way to study behavior. From the action of the same agent most diverse results follow, depending on the arrangement of the material on which it acts.

Second, we find, both in machines and in organisms, that a mass of substance may respond in the same way to reagents of the most diverse kind, reagents having varied and even opposite effects. The avoiding reaction in Paramecium may be caused by heat and by cold; by acids, by alkalies, and by salts; by electrolytes and by non-electrolytes; by increase of osmotic pressure and by decrease of osmotic pressure; by chemical action without change of osmotic pressure; by mechanical shock and by electric shock; and this is a type of what we find in organisms. How often of late in the history of comparative physiology have we seen a certain effect attributed to one specific physico-chemical principle after another,—always on the assumption that the direct physical action of the agent is the essential point. Increase in osmotic pressure is first heralded as the essential point, until we find that decrease in osmotic pressure has the same effect, and that chemical change without change in osmotic pressure has likewise this effect. Then positively charged ions are made responsible, till we find that negatively charged ions will produce the same effect. And so the gamut is run; till heat, cold, mechanical shock, and the most diverse agents are found acting in the same way.[1]

Parallel results are easily obtained from combinations of inorganic material. Imagine an arrangement such that when an electric connection is made, some characteristic action is per-

[1]For an example lying outside the field of behavior, the history of the theories of artificial parthenogenesis is most instructive.

formed; for example, a bell is rung. It is easy to so arrange that reagents of opposite character shall make the connection and so ring the bell. Thus the electric button could be placed on a support between and close to an upper and a lower post, contact with either of which completes the circuit. Now, heating the support would lengthen it and cause contact with the upper post, while cold would shorten it and cause contact with the lower post; in either case the bell would ring. Similarly, it would be easy to so arrange a circuit that addition of acid or alkali or any electrolyte would close it; or even so that non-electrolytes would have the same effect. By having a somewhat complex structure, the most varied reagents would all close the circuit and ring the bell. This is the sort of thing we find in organisms. From diverse reagents we may get the same reaction; from the same reagent we get sometimes one result, sometimes another.

It is this condition of affairs that has brought upon the head of the investigators that report it accusations from the narrower physico-chemists of indeterminism, of vitalism, and from the vitalists claims that physico-chemical explanations have failed.

Yet we find the same relations in the inorganic combinations that we call machines. In machines do the physico-chemical properties of the material determine its action? Is it proper to study these physical and chemical properties in order to understand how the machine acts?

Evidently these questions are to be answered *yes!* Only, we must study these properties intelligently, recognizing the fact that the properties due to a certain arrangement of material are among the most important of all, for it is to this arrangement that the specific action of the machine or the organism is due. From a mere study of the properties of unarranged iron, ivory, paint, ink, and of the action of various agents upon them, we could never understand the typewriter. So from a mere study of the unarranged material of organisms and the action of agents upon them, we can never hope to understand their behavior.

Herein lies the failure or inadequacy of much of the physico-chemical work along these lines; it has dealt mainly with the properties of unarranged material, when the arrangement is precisely the essential point. There is among certain general physiologists an intense prejudice against all morphology; a cardinal point of faith is that structure is of no account. By taking this stand, they play directly into the hands of the vitalists. Vitalistic theories flourish as a result of too simple statements of the problems, and too simple solutions, on the part of the physico-chemists. If we maintain that physico-chemical explanation means that behavior in

lower organisms is the direct result of simple chemical and physical action on a certain kind of substance,—then such explanation undoubtedly fails, and the vitalist triumphs.

What is most needed is that the physico-chemical student of biology shall realize that as matter takes on new arrangements, its activities and reactions become different, even though the properties of each constituent part may remain the same. Since in living things there are beyond doubt arrangements differing from anything found elsewhere, we are of course certain to find in living things ways of acting that differ from anything found elsewhere. Hence we cannot expect to find in the physics and chemistry of inorganic matter the full explanation of the activities of organisms; those who expect to do this are following a will-o'-the-wisp,—and this is certain from physico-chemical principles themselves. The physicist and chemist must study organisms in order to fully understand physics and chemistry, just as the biologist must study physics and chemistry in order to understand organisms.[1]

The arrangement of the material is then the essential point in determining behavior. What then becomes from the mechanistic standpoint the fundamental problem of behavior? Is it not the question of how these arrangements of material arise, and how they become changed? This property of taking on typical arrangements and of changing these arrangements is the fundamental and essential property of living matter that requires study for the physico-chemical understanding of behavior.

Of such study hardly a beginning has been made. With regard to the minute internal physical changes in the taking on of new arrangements, we have merely some faint suggestions from the rearrangement of particles in the hysteresis of

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[1] Some writers have applied the name vitalism to the idea that new methods of action arise when new combinations occur, taken in connection with the view that new combinations are found in living things; so apparently Radl (Geschichte Biologischer Theorien, vol. 1, p. 81). But such vitalism involves no new principle of explanation; it is based upon conditions found in chemistry and physics as decidedly as in biology. New methods of action arise when oxygen and hydrogen combine, producing water; new methods of action arise when a mass of brass and iron is arranged in the form of a clock. How then can it fail to be true in the case of organisms? The study of physics and chemistry is the study of the methods of action of matter and energy, whether simple or in combination. No new principle of explanation is involved simply because the combinations studied are those in living things. This view seems therefore to the writer far from a vitalistic one. On this point, see the excellent discussion by O. Hertwig, *Der Kampf um Kernfragen der Entwicklungs- und Vererbungslehre* (Jena, 1909), pp. 55-81; notably p. 80.

colloids. A little more has been done in tracing the external manifestations of these changes in the individual organism, through the study of changes of behavior in different physiological conditions, and in the formation of habits and associations. The most important part of the problem lies in the question of how diversities of arrangement arise during the racial history.

It is here that the problem of behavior opens into the more general problems of heredity, variation and evolution. This is the field that underlies the study of behavior. How does the living substance become modified in the lapse of time, so as to take on new arrangements, and therefore to behave differently? On this, if we stick to our ideal of demanding to observe the processes as they occur, little has been accomplished. Von Uexküll, with this ideal in view, feels that a correct statement is this: that all we know about this process is that it does not occur as Darwin thought it did.[1] This is the central physico-chemical problem for a causal understanding of behavior. Compared with it, the study in living things of the changes in surface tension, osmotic pressure, the nature of solution, and the like, though important in themselves, can give us little help, save as they may underlie and finally lead to a knowledge of the properties and laws in accordance with which living matter takes on and changes its method of arrangement. This central problem of behavior is likewise the central problem of all biology.

Thus the condition of affairs we have sketched does not lead to indeterminism; to belief in the failure of physico-chemical explanations; to vitalism. It merely leads us to perceive that the problem is more complex than has been supposed; that the fundamental question is that regarding the production of arrangements in the living substance, and that the solution of this problem lies in the future.[2]

We have spoken of vitalism. What we have thus far set forth bears upon the problems of anthropomorphism, and of finalism or teleology, almost as directly as it does upon vitalism. We need therefore to touch upon these but briefly.

2. Anthropomorphism signifies unjustifiably reading into lower organisms the characteristics, particularly the mental

[1] Von Uexküll: Studien über den Tonus, V. Zeitschr. f. Biol., vol. 50, p. 168.

[2] I need not say that this discussion is not presented as a "refutation" of any special brand of vitalism, for example, that of Driesch. For such a refutation the precise arguments set forth in support of vitalism would have to be taken up and overcome. I have here tried merely to set forth the attitude I have reached after some years of work on behavior.

ones, that we find in ourselves, and especially it signifies substituting these for a causal explanation. This is a serious error. But it has nothing to do with another question, with which it is often confused. This other question is, whether the behavior of animals resembles in any features the behavior of man. This is purely a question of objective fact, not one for prejudice or for *a priori* considerations of any sort. The only way to answer it is to learn the objective facts for man and for animals, and then to compare them, observing where there are resemblances, where differences. To see such an objective comparison proposed seems to arouse all the fighting instincts of some of our ultra physico-chemical friends. just as it does some of our theological friends. But if we persist in making it undisturbed, we undoubtedly find many fundamental resemblances, along with many differences, between the behavior of even the lowest organisms and of man. Some of these are: the fact that reactions are due to the release of internal energy; that action may occur without specific external stimulus; that action is modified by internal changes of the most varied character, many of which are parallel in man and protozoan; that negative reactions are given mainly to injurious agents, positive ones to beneficial agents; that varied reactions occur under the influence of a single constant stimulus, and that the organism tends to persist in that reaction which keeps it in conditions favorable to its life processes.

To point out these and similar resemblances is merely to point out the facts, with which we must all come to some sort of a working compromise. The existence of organisms whose behavior has these characteristics is not inconsistent with physico-chemical explanation, if physico-chemical explanation is valid, for man does exist. No question of fundamental principle is then involved when we find that other organisms have these characteristics; the question is merely as to the distribution of characteristics known to exist.

The problem of behavior then is: How are such characteristics to be explained from the physico-chemical point of view? If the practical physico-chemist declares that they cannot ultimately be so explained, he merely condemns his own methods of work and places himself among the vitalists. For such faint-heartedness there is surely as yet no justification! We have indeed to attack certain problems hardly yet approached, —the study of the physico-chemical properties in virtue of which certain materials become arranged in such characteristic ways as to produce the phenomena we have mentioned.

3. The matter of teleology or finalism is in similar case. We find as a mere matter of fact, certain marked relations be-

tween a present process and something that exists later. These relations resemble in many ways the relations between action that in man is accompanied by a purpose, and the result of that action. We find such relations in many fields besides behavior; the physiologist is compelled to recognize them everywhere. In the study of behavior it has become a sort of popular fad to ignore these relations; to act as if they did not exist. These are merely the tactics of the ostrich. We must face the problems which nature sets us. When the lens of the eye develops in the dark, it is mere cowardice to try to act as if we did not know that this lens is so constituted as to bring light to a focus. When the pancreas secretes an enzyme, it is again struthionic tactics to refuse to see the relation of that enzyme to the digestion of food,—although this digestion does not take place till after the secretion has occurred. Behavior in lower organisms is an almost continuous tissue of such relations; they are largely what we call the regulatory features of behavior.

When the ultra physico-chemist assumes that the pointing out of these extraordinary relations is given as an explanation of them, he shows most surprising naïveté. They form precisely the most difficult, the most complex, problem for causal explanation. The question is; How were such relations brought about? This is a question for precisely the same kind of objective investigation as the question how any other relation was brought about. How does the boulder happen to lie in the middle of the plain? How does the lens get into the optic cup? How does the lens happen to be of such a form as to bring light to a focus? How does the animal happen to react in such a way as to protect itself? The answer in every case lies in tracing the processes by which the relations were brought about, and in discovering the laws of these processes; by beginning with the condition where these relations do not exist, and tracing the "particular go" of the changes until these relations do exist.

How the relations that impress us as teleological were brought about constitutes undoubtedly a set of most difficult problems. But to keep us from despairing, we find this process taking place in the lives of individuals in a manner that can readily be studied. This is in the formation of habits. In the formation of habits, we see that the organism at first does not react in a way that impresses us as teleological, while later it does, and we can watch the process of change from one condition to the other, and discover how it is causally determined. Since then a method of action that appears to us teleological is produced in an intelligible way under our very eyes, in the lifetime of the individual, there is no reason why we

may not expect to find out how teleological relations have been brought about in the life of the race, when we have actually made a start in the study of the physiology of racial processes.[1] It seems clear that the apparent relation of a present process or structure to something that comes later in time is always due to the fact that this future something has in fact acted upon the organism in the past. The present condition fits the future condition only because of a certain constancy in the universe, through which the 'something past' reappears again in the future.

Let us then attempt in closing to characterize behavior as we find it in its lowest stages, recapitulating the chief points we have made. We find in lower organisms, as in higher animals, that the nature of the reactions is due mainly to characteristic *arrangements* of material, not to the properties of simple unarranged substance. These lower organisms therefore furnish problems which do not differ in kind from what we find in higher animals. They are simpler only in a numerical sense, — in that their parts are less numerous than those of higher animals. It would be most interesting if we found in these lower creatures a half way stage to inorganic matter; if we found living matter without characteristic arrangements, so that its properties and actions were those of an undifferentiated substance. But perhaps the chief result of research is to show that we do *not* find this condition realized. Doubtless this is a great disappointment, much diminishing the supposed importance of studying the lowest organisms. But we must bow to the facts.

In the behavior of these lower creatures we do not find that uniformity which certain physico-chemical theories of behavior demand. With certain underlying conditions in common, extreme diversities in methods of action are the rule. To the same stimuli different organisms react differently; different individuals of the same species react differently, and even the same individual reacts differently at different times. As Walter has well said "strictly speaking, all behavior is individual behavior,"[2] and Driesch[3] has shown his usual acumen in setting

[1] The statement is sometimes made that we can never hope to reduce teleological relations to causal relations (see v. Uexkull, Leitfaden in das Studium der experimentellen Biologie der Wassertiere, 1905, p. 129). This doubtless means that the teleological relations remain, even after the process by which they are brought about has been explained causally. It certainly cannot be maintained, in view of the known process of forming useful habits, that the process itself cannot be understood from a causal standpoint.

[2] Walter, H. E.: The Reactions of Planarians to Light. *Journ. Exper. Zoöl.*, 5, 1907, p. 97.

[3] Driesch, H.: The Science and Philosophy of the Organism, 1908-9.

forth individuality as the central problem of biology. For the purposes of physico-chemical explanation, individuality in behavior is an outgrowth of the fact that there is practically infinite diversity in the arrangement of living substance, so that no two specimens of it are precisely alike, therefore they do not act precisely alike. Behavior in lower organisms presents itself exactly as it would if the theory of descent with unlimited modifiability were true,—every individual forming a centre from which modifications may and do diverge in many directions. If we abandon for a moment our requirement of seeing evolution occur before we accept it, and assume that it *has* occurred, then we should hold that lower organisms are not really more primitive than higher ones; each may have as long a history and as many modifications behind it as a higher animal. They would be conceived merely as creatures that have retained certain numerically simple arrangements, because they thus fit some otherwise unoccupied nooks and crannies of the universe.

The great problem of behavior then, as for biology in general, is to work out those properties of living matter and of the environment, by which characteristic arrangements of material are produced and modified. In all these typical arrangements or structures that we call organisms, including the protozoan and man, there are certain common features in behavior; the pointing out of these common features is at times denounced as anthropomorphism. Again, in the behavior of these typical arrangements of material, we observe certain relations of present actions to later conditions; these relations we call teleological. But how these conditions and relations are brought about is essentially a physico-chemical problem, in the sense that we can study only the processes and configurations of matter and energy from which they result. The entire actual situation in behavior is that on which theories of vitalism have been based. It can truly be said that the condition of affairs found in behavior is very nearly that set forth by the vitalists as a basis for vitalism; their conception of the gross facts of behavior is more nearly accurate and adequate, from a descriptive standpoint, than is that set forth by the simplifying physico-chemists. The vitalists take into consideration all the phenomena, while the ultra physico-chemists leave out of account the most interesting ones.

But this does not mean that vitalism is anything more than a name for what we have not yet worked out; it does not mean the giving up of the ideal of essentially physico-chemical or mechanistic explanation. It means merely that we must recognize the enormous complexity and difficulty of the physico-

chemical problem;[1] must realize that we have hardly gotten hold of the first threads for unravelling the puzzle yet; that we have, indeed, hardly attacked the real problem, save in the mass and by analysis into components that are themselves inconceivably complex. The central problem, of working out the laws and processes by which typical arrangements of matter and energy are produced and modified in organisms, presents itself as a problem to be attacked only by the essentially physico-chemical methods through which all the real causal explanation that we have has thus far been reached.

[1] "Nature cannot be made simple by treating her on the theory that she ought to be so, when as a matter of fact she is not." Ritter, l. c., p. 187.

PSYCHOLOGICAL PROBLEMS IN ANTHROPOLOGY[1]

By Franz Boas

The science of anthropology deals with the biological and mental manifestations of human life as they appear in different races and in different societies. The phenomena with which we are dealing are therefore, from one point of view, historical. We are endeavoring to elucidate the events which have led to the formation of human types, past and present, and which have determined the course of cultural development of any given group of men. From another point of view the same phenomena are the objects of biological and psychological investigations. We are endeavoring to ascertain what are the laws of hereditary stability and of environmental variability of the human body. These may be recognized in the historical changes that the bodily appearance of man has undergone in the course of time, and in his displacement from one geographical or social environment to another. We are also trying to determine the psychological laws which control the mind of man everywhere, and that may differ in various racial and social groups. In so far as our inquiries relate to the last-named subject, their problems are problems of psychology, though based upon anthropological material. I intend to speak of this aspect of anthropology to-day.

The fundamental problem on which all anthropological inquiry must be founded relates to the mental equipment of the various races of man. Are all the races of mankind mentally equally endowed, or do material differences exist? The final answer to this question has not been given, but anatomical observations on the various races suggest that differences in the form of the nervous system are presumably accompanied by differences in function, or, psychologically speaking, that the mental traits which characterize different individuals are distributed in varying manner among different races; so that the composite picture of the mental characteristics of one race would presumably not coincide with the composite picture of the mental characteristics of another race. The evidence that has been brought forward does not justify us,

[1] Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

however, in claiming that the characteristics of one race would be an advance over those of another, although they would be different.

This question has also been approached from the standpoint of racial achievement. It has been pointed out that only the white race and the Mongolian race have reached any high grade of cultural development, and on this basis it has been assumed that the other races of man have not the ability to reach the same grade of civilization. It has been shown, however, that the retardation of the other races is not necessarily significant, because the amount of retardation is small as compared to the time consumed in reaching the present stages. It would seem, therefore, that the weight of evidence is, on the whole, in favor of an essential similarity of mental endowment in different races, with the probability of variations in the type of mental characteristics. Further inquiries into this subject must be based not only on sociological studies, but also on anatomical, physiological, and psychological inquiries among individuals belonging to the distinct races of mankind.

While the problem that I have just outlined relates to hereditary racial differences, a second fundamental problem of anthropology relates to the mental characteristics of social groups regardless of their racial descent. Even a superficial observation demonstrates that groups of man belonging to distinct social strata do not behave in the same manner. The Russian peasant does not react to his sense experiences in the same way as does the native Australian; and entirely different from theirs are the reactions of the educated Chinaman and of the educated American. In all these cases the form of reaction may depend to a slight extent upon hereditary individual and racial ability, but it will to a much greater extent be determined by the habitual reactions of the society to which the individual in question belongs.

The reaction of a member of a society to the outer world may be twofold. He may act as a member of a crowd, in which case his activities are immediately determined by imitation of the activities of his fellows; or he may act as an individual; then the influence of the society of which he is a member will make itself felt by the habits of action and thought of the individual.

I have discussed the racial question repeatedly at other places. The problem of the psychology of the crowd is a peculiarly intricate one, based largely upon the data of social psychology in a wider sense of the term, and upon data of individual psychology. I may be allowed for these reasons to confine myself to-day to the third of the problems which I have

outlined, that of the psychological laws which govern man as an individual member of society.

This problem has been the object of intensive study by the great minds that have laid the foundation of modern anthropology. The ultimate aim of Waitz's great work is the inquiry into the question whether there are any fundamental differences between the mental make-up of mankind the world over, racially as well as socially. Tylor, in his brilliant investigations on the development of civilization, showed the common occurrence of similar types of ideas the world over, and demonstrated the possibility of conceiving of the scattered phenomena as proof of certain tendencies of evolution of civilization. The many investigators who have studied the evolution of marriage relations, the evolution of law, of art, of religion, all start from the same basis—the assumption of a general similarity of mental reaction in societies of similar structure. Bastian has tried to prove by the use of anthropological data that man the world over develops the same elementary ideas, on which the fabric of his mental activities is based; and that these elementary ideas may be modified by geographical and social environment, but that they remain essentially the same everywhere.

It may be well to illustrate the facts here referred to by a few examples. In the domain of industrial activity we find that mankind is everywhere in possession of the art of producing fire by friction, that everywhere food is prepared by cooking, that shelters are built, that tools are used for breaking and cutting. We do not know mankind in any stage where any of these inventions are absent. In regard to social structure we find that man nowhere lives alone; that even the cases in which the social group consists of members of one family only, are exceedingly rare and of temporary occurrence. We furthermore find that the social units are subdivided into groups, which are kept apart by customary laws forbidding intermarriages in one group, and prescribing intermarriages in another.

In the domain of religion an idea of this type is that of life after death. There is probably no people that believes in the complete extinction of existence with death, but some belief in the continuity of life seems to exist everywhere. To the same domain belongs that type of concepts of the world, in which the surface of our earth is considered as forming a central level, above and below which other worlds are located.

An examination of the types of ideas represented by the few examples that I have here given shows that their subject-matter is highly complex, and that in a strict sense the occurrence of these ideas by itself does not explain clearly the psychological

processes that produce them and that cause their stability. Attempts at a psychological interpretation of these concepts have often been made by means of a comparative treatment of similar ideas, and by endeavors to arrange these ideas in such a way as to show a more or less rationalistic development of one from the other. While this may be feasible in some cases, it does not seem likely that this method of treatment will lead us to the most generalized laws governing the forms of thought in human societies.

The principal obstacle in the way of progress on these lines seems to my mind to be founded on the lack of comparability of the data with which we are dealing. When, for instance, we speak of the idea of life after death as one of the ideas which develop in human society as a psychological necessity, we are dealing with a most complex group of data. One people believes that the soul continues to exist in the form that the person had at the time of death, without any possibility of change; another one believes that the soul will be reborn in a child of the same family; a third one believes that the souls will enter the bodies of animals; and still others that the shadows continue our human pursuits, waiting to be led back to our world in a distant future. The emotional and rationalistic elements which enter into these various concepts are entirely distinct; and we can readily perceive how the various forms of the idea of a future life may have come into existence by psychological processes that are not at all comparable. If I may be allowed to speculate on this question, I might imagine that in one case the similarities between children and their deceased relatives, in other cases the memory of the deceased as he lived during the last days of his life, in still other cases the longing for the beloved child or parent, and again the fear of death—may all have contributed to the development of the idea of life after death, the one here, the other there.

Another instance will corroborate this point of view. One of the striking forms of social organization, which occurs in many regions wide apart, is what we call totemism,—a form of society in which certain social groups consider themselves as related in a supernatural way to a certain species of animals or to a certain class of objects. I believe this is the generally accepted definition of totemism; but I am convinced that in this form the phenomenon is not a single psychological problem, but embraces the most diverse psychological elements. In some cases the people believe themselves to be descendants of the animal whose protection they enjoy. In other cases an animal or some other object may have appeared to an ancestor of the social group, and may have promised to become his protector, and the friendship between the animal and the ancestor

was then transmitted to his descendants. In still other cases a certain social group in a tribe may have the power of securing by magical means and with great ease a certain kind of animal or of increasing its numbers, and the supernatural relation may be established in this way. It will be recognized that here again the anthropological phenomena, which are in outward appearances alike, are, psychologically speaking, entirely distinct, and that consequently psychological laws covering all of them can not be deduced from them.

Another example may not be amiss. In a general review of moral standards we observe, that, with increasing civilization, a gradual change in the valuation of actions takes place. Among primitive man human life has little value, and is sacrificed on the slightest provocation. The social group among whose members any altruistic obligations are binding is exceedingly small; and outside of the group any action that may result in personal gain is not only permitted, but even approved; and from this starting point we find an ever-increasing valuation of human life and an extension of the size of the group among whose members altruistic obligations are binding. The modern relations of nations show that this evolution has not yet reached its final stage. It might seem, therefore, that a study of the social conscience in relation to crimes like murder might be of psychological value, and lead to important results, clearing up the origin of ethical values; but I think here the same objections may be raised as before, namely the lack of comparable motives. The person who slays an enemy in revenge for wrongs done, a youth who kills his father before he gets decrepit in order to enable him to continue a vigorous life in the world to come, a father who kills his child as a sacrifice for the welfare of his people, act from such entirely different motives, that psychologically a comparison of their activities does not seem permissible. It would seem much more proper to compare the murder of an enemy in revenge, with destruction of his property for the same purpose, or to compare the sacrifice of a child on behalf of the tribe with any other action performed on account of strong altruistic motives, than to base our comparison on the common concept of murder.

Similar observations may also be made in the domain of art. The artist who tries to display his skill in handling his material will be led to æsthetic results. Another one, who wishes to imitate certain forms in his work, may be led to similar results. Notwithstanding similarity of results, the psychological processes in these two cases are quite distinct and not comparable.

For these reasons it seems to me that one of the fundadamental points to be borne in mind in the development of anthropological psychology is the necessity of looking for the

common psychological features, not in the outward similarities of ethnic phenomena, but in the similarity of psychological processes so far as these can be observed or inferred.

Let us next consider in what direction the psychological problems of anthropology have to be looked for. I must confine myself here to a very few examples of what seem to me fundamental psychological facts.

One of the most striking features in the thoughts of primitive people is the peculiar manner in which concepts that appear to us alike and related are separated and re-arranged. According to our views the constituting elements of the heavens and of the weather are all inanimate objects; but to the mind of primitive man they appear to belong to the organic world. The dividing-line between man and animal is not sharply drawn. What seem to us conditions of an object—like health and sickness—are considered by him as independent realities. In short, the whole classification of experience among mankind living in different forms of society follows entirely distinct lines. I believe this subject can be made clear most easily by a comparison with a similar phenomenon in languages.

If the whole mass of concepts, with all their variants, were expressed in language by entirely heterogeneous and unrelated sound complexes, a condition would arise in which closely related ideas would not show their relationship by the corresponding relationship of their phonetic signs. An infinitely large number of distinct sound complexes—in other words, of distinct words—would be required for expression. If this were the case, the association between an idea and its representative sound complex would not become sufficiently stable to be reproduced automatically at any given moment, without reflecting. The automatic and rapid use of language has brought it about that the infinitely large number of ideas have been reduced by classification to a lesser number, which by constant use have established firm associations, and which can be used automatically. It seems important to emphasize the fact that the groups of ideas expressed by specific words show very material differences in different languages, and do not conform by any means to the same principles of classification. To take the example of English. We find that the idea of water is expressed in a great variety of forms. One term serves to express water as a liquid; another one, water in the form of a large expanse, a lake; others, water as running in a large body or in a small body, a river and brook. Still other terms express water in the forms of rain, dew, wave, and foam. It is perfectly conceivable that this variety of ideas, each of which is expressed by a single independent term in English, might be expressed in other lan-

guages by derivations from the same term. It seems fairly evident that the selection of simple terms must to a certain extent depend upon the chief interests of a people; and where it is necessary to distinguish a certain phenomenon in many varieties, which in the life of a people play each an entirely independent rôle, many independent words may develop, while in other cases modifications of a single term may suffice. In the same way as concepts are classified and groups of perceptions are expressed by a single term, relations between perceptions are also classified. The behavior of primitive man makes it perfectly clear that all these linguistic classes have never risen into consciousness, and that consequently their origin must be sought not in rational, but in entirely unconscious processes of the mind. They must be due to a grouping of sense impressions and of concepts which is not in any sense of the term voluntary, but which develops from entirely different psychological causes. It is a characteristic of linguistic classifications that they never rise into consciousness, while other classifications, although the same unconscious origin prevails, often do rise into consciousness. It seems very plausible, for instance, that the fundamental religious notions, like the idea of will power immanent in inanimate objects, or the anthropomorphic character of animals, are in their origin just as little conscious as the fundamental ideas of language. While, however, the use of language is so automatic that the opportunity never arises for the fundamental notions to emerge into consciousness, this happens very frequently in all phenomena relating to religion.

I believe that anthropological investigations carried on from this point of view offer a fruitful field of inquiry. The primary object of these researches would be the determination of the fundamental categories under which phenomena are classified by man in various stages of culture. Differences of this kind appear very clearly in the domain of certain simple sense-perceptions. For instance, it has been observed that colors are classified according to their similarities in quite distinct groups without any accompanying difference in the ability to differentiate shades of color. What we call green and blue are often combined under some such term as "gall-like color," or yellow and green are combined into one concept, which may be named "young-leaves color." The importance of the fact that in thought and in speech these color-names convey the impression of quite different groups of sensations can hardly be over-rated.

Another group of categories that promise a field of fruitful investigation are those of object and attribute. The concepts of primitive man make it quite clear that the classes of ideas

which we consider as attributes are often considered as independent objects. The best-known case of this kind, one to which I have referred incidentally before, is that of sickness. While we consider sickness as a condition of an organism, it is believed by primitive man, and even by many members of our own society, to be an object which may enter the body, and which may be removed. This is exemplified by the numerous cases in which a disease is extracted from the body by sucking or by other processes, in the belief that it may be thrown into people, or that it may be enclosed in wood in order to prevent its return. Other qualities are treated in the same way. Thus the condition of hunger, exhaustion, and similar bodily feelings, are considered by certain primitive tribes as independent objects which affect the body. Even life is believed to be a material object that may become separated from the body. The luminosity of the sun is considered as an object that the Sun himself may put on or lay aside.

I have indicated before that the concept of anthropomorphism seems to be one of the important categories underlying primitive thought. It would seem that the power of motion of the self and the power of motion of an object have led to the inclusion of man and movable objects in the same category, with the consequent imputation of human qualities to the moving objective world.

While in many cases we can see with a fair degree of clearness the fundamental concepts underlying these categories, in other cases these are not by any means clear. Thus the concept of incest groups—those groups in which intermarriage is strictly forbidden—is omnipresent. But no satisfactory explanation has so far been given for the tendency to combine certain degrees of blood relationship under this view-point.

Much material for this field of inquiry is contained in the works on comparative anthropology, but I believe a more thorough psychological analysis of the accumulated data may reveal important new information.

We will now turn to the consideration of another group of psychological phenomena that seem to me of considerable importance. In all forms of society certain groups of activities and of thoughts appear in certain typical associations. Thus in our modern society the consideration of cosmic phenomena is constantly associated with the efforts to give adequate explanations for them, based on the principle of causality. In primitive society the consideration of the same phenomena leads to a number of typical associations which differ from our own, but which occur with remarkable regularity among tribes living in the most remote parts of the world. An excellent instance of this kind is the regular association of obser-

vations relating to cosmic phenomena with purely human happenings; in other words, the occurrence of nature myths. It seems to my mind that the characteristic trait of nature myths is the association between the observed cosmic events and what might be called a novelistic plot based on the form of social life with which people are familiar. The plot as such might as well develop among the peoples themselves; but its association with the heavenly bodies, the thunder-storm, or the wind, makes it a nature myth. The distinction between the folk-tale and the nature myth lies solely in the association of the latter with cosmic phenomena. This association does not naturally develop in modern society. If it is still found every now and then, it is based on the survival of the traditional nature myth. In primitive society, on the other hand, it is found constantly. The investigation of the reason for this association is an attractive problem, the solution of which can only in part be surmised.

A number of other examples will demonstrate that the kind of association here referred to is quite common in primitive life. An excellent instance is furnished by certain characteristics of primitive decorative art. With us almost the sole object of decorative art is æsthetic. We wish to beautify the objects that are decorated. We recognize a certain appropriateness of decorative motives in accordance with the uses to which objects are to be put, and the emotional effect of the decorative motive. In primitive life the conditions are quite different. Extended investigations on decorative art in all continents have proved that practically everywhere the decorative design is associated with a certain symbolic significance. There is hardly a case known where a primitive tribe cannot give some sort of explanation for the designs they use. In some cases the symbolic significance may be exceedingly weak, but ordinarily it is highly developed. The triangular and quadrangular designs of our Plains Indians, for instance, almost always convey definite symbolic meanings. They may be records of warlike deeds, they may be prayers, or they may in some way convey other ideas relating to the supernatural. It would almost seem that among primitive tribes decorative art for its own sake does not exist. The only analogies in modern decorative art are such as the use of the flag, of the cross, or of emblems of secret societies, for decorative purposes; but their frequency is insignificant as compared to the general symbolic tendencies of primitive art. Thus it will be seen that we have here again a type of association in primitive society quite different from the type of association found among ourselves. Among primitive people the æsthetic motive is combined with the symbolic,

while in modern life the æsthetic motive is either quite independent, or associated with utilitarian ideas.

I will give still another example of a form of association characteristic of primitive society. In modern society, social organization, including the grouping of families, is essentially based on blood relationship and on the social functions performed by each individual. Except in so far as the Church concerns itself with birth, marriage, and death, there is no connection between social organization and religious belief. These conditions are quite different in primitive society, where we find an inextricable association of ideas and customs relating to society and to religion. I have referred before to the phenomena of totemism, which are perhaps the best example of this type of association. Totemism is found among many American tribes, as well as in Australia, Melanesia, and in Africa. I have described before its characteristic trait, which consists in supernatural connection that is believed to exist between a certain class of objects, generally animals, and a certain social group. Further analysis shows very clearly that one of the underlying ideas of totemism is the existence of definite groups of man that are not allowed to intermarry, and that the limitations of these groups are determined by considerations of blood relationship. The religious ideas found in totemism refer to the personal relation of man to certain classes of supernatural powers, and the typical trait of totemism is the association of certain kinds of supernatural power with certain social groups. Psychologically, therefore, we may compare totemism with those familiar forms of society in which certain social classes claim privileges by the grace of God, or where the patron saint of a community favors its members with his protection. It will be recognized that we have here again a type of association in primitive society which has completely changed with the development of civilization.

We will now turn to the consideration of a third point, to the peculiar importance of automatic actions in the development of the customs and beliefs of mankind. It is a well-known fact that all those actions which we perform with great frequency are liable to become automatic; that is to say, that their performance is ordinarily not combined with any degree of consciousness. Consequently the emotional value of these actions is also very slight. It is, however, remarkable that the more automatic an action, the more difficult it is to perform the opposite action; that it requires a very strong effort to do so; and that ordinarily the opposite action is accompanied by strong feelings of displeasure. It may also be observed that to see the unusual action performed by another person excites the strongest attention and causes feelings

of displeasure. An example will make clear what I mean. When we consider our table manners, it will readily be recognized that most of them are purely traditional and cannot be given any adequate explanation. Still the constant performance of the actions which constitute good table manners makes it practically impossible for us to act otherwise. An attempt to act differently would not only be difficult on account of the lack of adjustment of muscular motions, but also on account of the strong emotional resistance that we should have to overcome. To eat with people having table manners different from our own seems to us decidedly objectionable and causes feelings of displeasure which may rise to such intensity as to cause qualmishness. Another good example is the feeling connected with acts that in our society are considered as modest or immodest. Every one will feel instinctively the strong resistance that he would have to overcome, even in a different society, if he were required to perform an action that we are accustomed to consider as immodest, and the feelings that would be excited in his mind if he were thrown into a society in which the standards of modesty differed from our own. It seems to my mind that these feelings of displeasure exert a very strong influence upon the development and conservation of customs. The young child in whom the habitual behavior of his surroundings has not yet developed will acquire much of this behavior by unconscious imitation. In many cases, however, it will act in a way different from the customary behavior, and will be corrected by its elders. This is presumably one of the most important elements that tend to bring customary behavior into the consciousness of the people practising it. When educating their children to conform to the tribal standards, these standards must necessarily become conscious to the educators.

One of the cases in which the development of ideas based on behavior is best traced, is that of the taboo. Although we ourselves have hardly any definite taboos, to an outsider our failure to use certain animals for food might easily appear from this point of view. Supposing an individual accustomed to eating dogs should inquire among us for the reason why we do not eat dogs, we could only reply that it is not customary; and he would be justified in saying that dogs are tabooed among us, just as much as we are justified in speaking of taboos among primitive people. There are a number of cases in which it is at least conceivable that the older customs of a people, under a new surrounding, develop into taboos. I think, for instance, that it is very likely that the Eskimo taboo forbidding the use of caribou and of seal on the same day may be due to the alternating inland and coast life of the people. When they hunt inland, they have no seals, and consequently can eat only

caribou. When they hunt on the coast, they have no caribou, and consequently can eat only seal. The simple fact that in one season only caribou can be eaten, and that in another season only seal can be eaten, may have easily led to a resistance to a change of this custom; so that from the simple fact that for a long period the two kinds of meat could not be eaten at the same time developed the law that the two kinds of meat must not be eaten at the same time. I think it is also likely that the fish taboo of some of our Southwestern tribes may be due to the fact that the tribes lived for a long time in a region where no fish was available, and that the impossibility of obtaining fish developed into the custom of not eating fish.

It would seem, therefore, that we may say in a general way that the customary action is the ethical action, that a breach of custom is everywhere considered as essentially unethical.

It is very likely that the same causes have had a strong influence upon the development of local conventional styles of art. It is no less true that the customary form is liable to be considered the beautiful form than that the customary behavior is considered ethical behavior. Therefore the stability of primitive styles of art may ultimately be due to the same causes as the stability of primitive customs.

If the origin of concepts and of distinct types of association is such as I suggested to-day, and if the existence of these concepts and types of association is brought into the consciousness of primitive man by the incidents of his daily life, when customary concepts and customary associations seem to be broken, we recognize that man must in a great many cases find himself confronted with the fact that certain ideas exist in his mind for which he cannot give any explanation except that they are there. The desire to understand one's own actions, and to get a clear insight into the secrets of the world, manifests itself at a very early time, and it is therefore not surprising that man in all stages of culture begins to speculate on the motives of his own actions.

As I have explained before, there can be no conscious motive for many of these, and for this reason the tendency develops to discover the motives that may determine our customary behavior. This is the reason why in all stages of culture customary actions are made the subject of secondary explanations that have nothing to do with their historical origin, but which are inferences based upon the general knowledge possessed by the people. I think the existence of such secondary interpretations of customary actions is one of the most important anthropological phenomena, and one which is hardly less common in our own society than in more primitive societies. It is a common observation that we desire or act

first, and then try to justify our desires and our actions. When, on account of our early bringing up, we act with a certain political party, most of us are not prompted by a clear conviction of the justice of the principles of our party, but we do so because we have been taught to respect it as the right party to which to belong. Then only do we justify our standpoint by trying to convince ourselves that these principles are the correct ones. Without reasoning of this kind, the stability and geographical distribution of political parties as well as of church denominations would be entirely unintelligible. A candid examination of our own minds convinces us that the average man in by far the majority of cases does not determine his actions by reasoning, but that he first acts, and then justifies or explains his acts by such secondary considerations as are current among us.

That the same conditions prevail to even a greater extent among primitive people can easily be shown by a number of examples. It has been pointed out before that decorative art among primitive people is almost everywhere symbolic. This does not preclude the possibility of designs, and even of the whole style, of one region being borrowed by the people of another region. This has been the case, for instance, among the tribes of our Northwestern Plains, who have borrowed much of their art from their more southern neighbors; but they have not adopted together with it the symbolical interpretations given by their neighbors, but invented interpretations of their own. I imagine that this is the outcome of a mental process which set in when the designs were found pleasing, and, according to the general character of primitive thought, a symbolic interpretation was expected. This was then secondarily invented in accordance with the ideas current among the tribe.

The same observation may be made in primitive mythology. The same kind of tales are current over enormous areas, but the mythological use to which they are put is locally quite different. Thus an ordinary adventure relating to the exploits of some animal may sometimes be made use of to explain some of its peculiar characteristics. At other times it may be made use of to explain certain customs, or even the origin of certain constellations in the sky. There is not the slightest doubt in my mind that the tale as such is older than its mythological significance. The characteristic feature of the development of the nature myth is, first, that the tale has associated itself with attempts to explain cosmic conditions—this has been referred to before—and, secondly, that when primitive man became conscious of the cosmic problem, he ransacked the entire field of his knowledge until he happened

to find something that could be fitted to the problem in question giving an explanation satisfactory to his mind. While the classification of concepts, the types of association, and the resistance to change of automatic acts, developed unconsciously, many of the secondary explanations are due to conscious reasoning.

In the preceding remarks I have tried to point out a direction in which anthropological data may be used to good advantage by the psychologist; that from a psychological point of view, the starting-point of our investigations must not be looked for in anthropological phenomena that happen to be alike in outward appearance, but that in many cases diverse phenomena are based on similar psychic processes, and that these offer to the investigator a promising line of attack.

THE DYNAMIC INTERPRETATION OF DEMENTIA PRÆCOX[1]

By ADOLF MEYER

Up to recent years the ambition of scientific medicine was to trace all morbid conditions to some kind of anatomical lesion. This inevitably left a large field in which there was 'no pathology as yet,' and therefore a suspicion of inevitable chaos. The trend of the last decade and the experience with biological serum-reactions and especially also the progress of psychiatry has, however, greatly strengthened a *functional* and *biological* view of the events in living beings, so that the work of *pathology* appears to us primarily as the *determination of causal chains or conditions with the accuracy of an experiment*, and the lesions then take their place among the simple facts or symptoms, according to the extent to which they can be understood in terms of dynamic developments, *i. e.*, of cause (or conditions) and effect.

This functional way of seeing things has the great advantage of allowing us to arrange the facts as we see them.

Most attempts at translation of the functional facts into neurological homologies leave out the actual amount and duration of the function, and the all-important laws of compatibility and incompatibility of sequences, and the discrimination between what are *moving factors* or more decorative staging or incidents. Turning to the functional and dynamic conceptions allows us to remain true to neurological conceptions where we are entitled to any and yet to take in the entire psychological setting without which the events would be difficult to grasp, artificial and devoid of the spark of life and interest to most physicians.

The dynamic conception which I shall present in this lecture is not *new* in one sense, but in another it *reconquers* and makes safe the ground of common-sense psychology abandoned by medicine during the struggle against superstitions and also under the influence of philosophical speculations, now become indispensable again if we wish to bring differentiation into our field and account for, and explain, the

[1] Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

events constituting mental disorders. I have been said to resurrect the layman's opinion of the causes and nature of mental disease. I grant that to some extent; but we are also adding the *conditions* and critical safeguards under which we are justified in accepting and pursuing our normal instinctive interpretations.

Among mental diseases certain chains of events recur with such regularity that they become valuable clinical units or reaction-types.[1] When the development appears natural and plausible we consider the disorder accounted for and, under certain conditions, explained. In this respect, we find for instance that the so-called organic dementias are accounted for and to some extent explained by the extent and kind of brain disease, determination of which tells us what the symptoms and the development must have been or would probably be. In a more functional field, we find the deliria of intoxication, perhaps not explained, but empirically accounted for by a sufficient amount of intoxicant material *and* personal disposition. In a second place we may rank certain depressions and excitements and paranoic developments of which we may say that they come nearer and nearer being both explained and accounted for psycho-biologically; when we have all the facts, they are apt to rank fairly plainly as exaggerations of relatively normal reactions, as is the case with many depressions and delusional states. In a third place there are, however, disorders which seem to defy both explanation and accounting for; among these figure, according to the claims of many physicians, certain disorders prominent in the *dementia præcox* group. In order to be on concrete ground, I shall now briefly state a few of the types to be compared and discussed; and in view of the fact that we have with us the most fruitful workers on the problems of detail, I have chosen to limit myself especially to the broader settings in cases which anybody can find and study and form his own impressions and estimates on.

The following sketches form the material of our discussion:

A young man said to have been bright at school and in his early environment is transplanted from the South to New York at 22 (1903), fails to make friends, becomes morose and morbid and seclusive, especially after finally losing his position (Christmas, 1904); he resorts to quack-treatment for sexual neurasthenia, and finally goes to a general hospital with vague complaints of general weakness (Spring of 1905). Then, at home for six weeks, he showed *more* seclusiveness, was noticed to smile and talk to himself in an unexplained manner, imagined he was followed, in other words had the feeling of being at a disadvantage, and of mistrust and day-dream-like reac-

[1] *The Psychological Bulletin*, Vol. V, No. 8, p. 245. The Problems of Mental Reaction-types, Mental Causes and Diseases.

tions, not clearly sized up by the family, until May 31, 1905, after a restless and probably sleepless night, a tantrum precipitated the recognition of the condition. The young man came to breakfast unkempt, ate, then suddenly got up, shook hands with his mother, said she was the best mother he ever had and announced: 'Between you and me and E (sister) I am going to be President of the United States.' The mother was (probably quite unnecessarily) frightened, he tried to hold her; she escaped and he then shouted and broke up the furniture and the chandelier. When he came to the hospital he regretted the violence, said he did not know at the time what he was doing; 'It seemed like a feeling took control of me.' He remembered it all clearly, and added that at times he felt as if *somebody* was taking something out of him when they spoke, 'some of my mind, my intellect, my power'—feelings and judgments very characteristic of this type of dissociation of the personality. At the hospital he was able to occupy himself for a while but gradually grew more irritable and offish, more and more dull and finally passed into a classical negativistic stupor (with catalepsy, refusal of food and mutism) from which he rose to some extent in the course of a few years, but with decided dilapidation, gross indifference, tendency to lounging about, with an occasional semblance of distractibility and flight of talk, but absence of any spontaneous push or interests—a classical instance of terminal dementia.

Heredity is denied in this case. Early masturbation stands in the centre of a distinct bias; circumstances and this bias encouraged solitude, hypochondriacal ruminations, feelings of disadvantage, neglect of himself, a shirking of the usual balancing helps offered by the common habits of social and practical life of the environment; then came more positively morbid reactions: mistrust, ideas of being followed, and silent mulling over thoughts never brought to any control by action, or even to simple open ventilation, as shown in the smiling and muttering to himself. Of this long period we only know the further frustration of interest in even the ordinary personal attentions and ambitions, and a peculiar notion that the doctor and nurses stole his soul (probably an imagination partly based on the feeling of disadvantage in comparisons of his empty state with the healthy, vigorous and superior persons with whom he had forced and otherwise dodged contact), and the episodes of probably very empty compensatory pleasurable moods of mere smiles, and mutterings of matters enjoyed by himself but probably less and less fit to be spoken out, less likely to stand the criticism that comes to outspoken words and to stimulate desire for communication. Finally we see the culmination in a sleepless night, and the tantrum at breakfast which felt like a discharge of tension, forced, 'like a feeling took hold' of him, an action by something not quite himself, and in it a characteristic rise to a *contrast* and *compensation* where concrete life brought nothing but failures; and finally a complete avoidance of the trial method which might have balanced his ruminations. Then comes further evidence of queer interferences of thought; some of his mind, his intellect, his power was taken from him—a very frequent interpretation of defect of integration, of blocking and of interferences felt in these states of insufficiency and incongruity of thought and action. Finally there is the development of a most protracted and biologically elementary shrinking into himself with the classical catatonic stupor in which the patient is a mere bundle of tense inactive self-defense, and, after the relaxation of the tension, an emersion of only the most inferior residuals of his interests and stock of endowment and training—a scattered, dilapidated condition rather than

an elementary dementia; inadequacy of emotions and interests and corresponding superficiality and reduction of volition and activity.

I pass over a similar case in which I originally (1905) formulated the evolution of the psychoses in terms of habit-conflicts which shows even more clearly the curve of transformation: masturbation from 9; decline of efficiency at school from 11, at the same time desire to become a teacher, mainly on account of the neatness of dress; frequent headache; two attempts at outside employment, then retired and rather secluded existence at home, excessive shame over the development of a small patch of gray hair; headaches and sleepiness in the morning; later ideas of having a tape worm; an operation for hemorrhoids at 21, with fear that her rectum would close up; then amenorrhœa with worry over her sexual misdeeds; futile attempts to make a better start; she bought paper and pens to become a book-keeper, but never got further; she would insist that she was a 'good girl;' she began to sleep poorly, became afraid at night, and at a party she complained that everybody looked at her. Then came a peculiar religious zeal; she poured kerosene oil on the steps for holy water; she ran to church at midnight; at Bellevue hospital she said—'I hear angels telling me how to pray when I lose my thought'—evidently a happy interpretation of the blocking and feeling of automatism; she scalded her right arm to save the world, soiled herself ('I had to be once more a baby'), developed a peculiar religious-symbolic talk and attitude, at times with senseless moralizing and ecclesiastic utterances with an undercurrent of salvation achieved or longed for, for herself and the world, and passed into a semi-ecstatic catatonic stupor which relaxed after a few years but left the patient in an apathetic so-called 'terminal' state, in the absence of any adequate balancing material before the attack. Again habit conflicts, gradual decline of efficiency balanced by empty ambitions, a series of incomplete attempts to rise, seclusion, hypochondriasis, finally a religious-fantastic compensation of catatonic form and lack of reintegration.

Another type in which the element of tension did not enter in the same manner is that of a young woman stenographer, committed at the age of 21, the child of a thriftless father, and sister of a young man who passed through a transitory mental upset at 20, and two relapses since; the patient had spent several years at a protectory, was bright in her studies, took up dressmaking and shortly after stenography and typewriting. She became, however, indifferent and lazy, remained in bed as late as possible in the morning. She became discontented at home, induced her mother to move into apartments beyond their means and asked her for money to take dancing lessons to go on the stage. October, 1906, she was dismissed from her position because of tardiness in reaching the office. She did not care, tried other positions but could not hold them. When in April, 1907, her brother was sent to a hospital, she became very seclusive, did not leave the house except for business or church. She said later that at that time she learned that her brother masturbated. She read more and more theatrical journals and romantic stories, bought peroxide to bleach her hair, and a preparation to remove the hair from her arms. During the summer of 1907 she spoke occasionally of a young Jew in the office paying attention to her. One night she awakened from a dream screaming; she told her mother that the day before, this young man had put his arms around her neck. She got impure thoughts, confessed having long indulged in masturbation; she admitted that she had thought of this young man in her dream. She gave up her position, feared the men would see her shame in her eyes. Her mother then noticed that her mouth and nose twitched in a peculiar way,

she appeared depressed, threatened to end her life; she was taken to a general hospital where she remained four days and was then transferred to Bellevue and finally to our service.

On admission the patient was quiet, spoke in a subdued voice; at times her expression was rather anxious but she could be easily induced to smile; there was much twitching of the upper lip with dilatation of the nostrils, and quivering of the brow muscles; she did not seem to be afraid yet she uttered many apprehensive ideas and expressed much fantastic horror over the surroundings, asked if they would choke her and bind her down or put her in a cage and make her walk on all-fours. She talked quite freely and often complained of her mind wandering; of inability to concentrate, used many but half-understood words, and remarked that she was not talking connectedly. She referred the talk of other patients to herself and spoke many times of hearing voices. When the nurse rattled her keys, she remarked: "I hear chains—I know I was blind and my brain is turned at the present moment." When asked what her difficulty was she said: "The thing that is uppermost in my mind is a fear that my mother and my brother will go insane if I die—if I—if I am fumigated—this isn't a very connected speech is it?" When asked to explain what she meant by fumigated she remarked: "It means burned up, don't it and thrown to the four winds—you are writing down all I say." When she heard some patients talking in the sitting hall she remarked: "Is n't that terrible those voices out there—that woman is saying I am insane—after this she will punish me—do something terrible to me—put me in a cage and I will have no shame left."

She told of the young man in the office who had put his arms around her and said that later she had a dream in which this young man tried to 'hurt' her; she resisted; after that she had the idea that the young man knew what she had dreamt about. Before she had the dream she had seen this young man make lewd motions and he had winked at her. She thought she had ruined herself by masturbation—"I was ashamed to look at any man—I thought they saw what I had done in my eyes."

The patient was clear as to her whereabouts and her memory was good: she did calculations correctly and promptly.

Her general physical condition was good; she was menstruating when admitted.

Following her admission she was languid, expressed many apprehensive ideas, but did not appear to be in any fear; she complained a great deal of hearing voices, but it was difficult to get her to specify what she heard. She continued to complain that her mind wandered, things were mixed up, she could not concentrate and admitted that bad thoughts kept coming into her mind. Once she asked the physician: "Can't you throw something on me like water to wake me up—I don't know if I am dreaming or not—I used to be tidy but now I am horrible, I don't take care of my person any more." Another time she asked: "Am I crazy, will I ever get over this?" When questioned she was inclined to cover her face, she wished to avoid the physician and expressed feelings of shame.

She was able to do the thinking tests without any special difficulty. grasped the point of what she read fairly readily, but occasionally she gave rather peculiar answers when questioned about her school knowledge and her thoughts at times seemed to be quite scattered.

While in her general demeanor she appeared dejected she never spoke of being sad and she frequently smiled even when uttering anxious ideas—*e. g.*, she remarked with a smile: "I am afraid of being killed." She complained a great deal of feeling strangely, she talked

of shocks of electricity going through her body and of little strings running through her skin; very often she referred to such ideas as 'imaginations.' She spoke of talk from inside of her body, voices making lewd suggestions in regard to Christ; she was afraid to repeat what she heard fearing that she would go to hell. She also heard voices from the outside, they told her she would drop dead; she thought it must be wireless telegraphy. She said in regard to the voices—"my brain seems to turn with them, I have no will power."

After a month in the hospital she had become extremely languid and apathetic; notwithstanding continued efforts to gain her interest and to crowd out the ruminative lapses she could not be induced to occupy herself in any way and was very careless about her appearance. Frequently she would throw herself on the floor, whimper and talk of dying. She explained this as a reaction to voices which suggested that she should drop dead. Once she referred to her behavior in Bellevue: "I was very violent and I thought I was Jesus—I jumped on the radiator and sang, thought I was a bird and a parrot." Once she suddenly sprang up from her chair and said that people downstairs were pulling strings; another time she was seen standing by the table running her fingers along the edges; when asked to explain this she said: "*It's all braining*."

During November, 1907, the patient appeared a little brighter, but she was still extremely languid, listless and apathetic; she expressed many strange ideas and peculiar feelings—*e. g.*, "It seems that people get possession of me in some way through my ears." When asked what she was occupied with she replied: "Something twisted my brain —these people that walk around talk with their feet—my brains have floated down my back" (smiling). When asked if her thoughts were connected she replied: "No, they fly around as though they go out of my ears and float."

Once during the interview she suddenly began to moan, breathed deeply and put her head down on the table. When asked to explain this behavior she said that she felt something in her cheek like 'meat' and thought she heard a young man's name mentioned, a butcher whom she had previously been interested in.

At a later occasion she gave a long statement difficult to understand except as a mixture of reminiscence and sexual symbolism. At present she is largely absorbed by her scattered ruminations, with excessive and forced play of her features. She does not utter her dream-life above a whisper, and in fragments; is unapproachable, and only rises to the level of communication in asking for her hat to go home to her mother, giving the correct address.

Here hereditary deficiency, deterioration of concrete interests with ill-founded and ill-directed aims, and a total missing of her level, with early masturbation and sexual ruminations and poorly controlled ambitions, led away from concrete productivity and from the checks and props of helpful environment and finally to a break of compensation in a sexual experience and a dream, and ideas that men could see her shame, then a pseudo-compensation by fantastic and partly dissociated or hallucinatory (disowned) ruminations in the form of voices and thoughts of uncontrollable and lewd contents, at the same time with queer feelings about her brain, growing preponderance of stilted words, facial expression with exaggerated grimaces and twitchings, discrepancy of mood and thought (laughing when saying that she was afraid of being killed), and a gradual crowding out of the normal thinking and grasp on facts and tendencies by the wholly demoralized gushes of fancy and mental self-abuse. No interests to which to appeal;—progressive deterioration.

While *these* disorders are mainly examples of a gradual and diffuse *deterioration of concrete interests*, the following case presents a striking evolution of a side-tracked complex of specific *longings* to the point of a compensatory fulfillment. A Jewess free of heredity, of rather perverse and stubborn disposition and with outbursts of temper as a child, became an efficient dressmaker, married at 23 a rather inferior man, and was an excellent and efficient wife but excessively jealous. She was crazy for children, but remained sterile. Treatment in 1892 and again 1902 availed nothing; 1902, when run down from a septic infection of the hand, she was told she could never have any children and was greatly upset. She was then subject to vivid *dreams* in which she was attacked. A few months later, a physican suggested an operation for uterine tumor, but she made a scene, called him a bandit and a murderer. Soon after that she began to disclose that she was sure the physician who treated her in 1892 had removed a child from her as she 'was then pregnant.' She said she had seen her boy in the park, asked newsboys to find her Benny, and finally upbraided a physician for *keeping* her child for experimental purposes and was arrested. At the hospital she repeated these fancies in a perfectly orderly fashion, gradually began to promise to let the matter drop and in two months she was taken home. Three months later she resumed her accusations, claimed she was being operated on every night, that doctors damaged her brain away; the papers pictured her as the mother of imagination. She was returned, thought she was pregnant, had nocturnal hallucinatory experiences, spoke of conspiracy, and said the nurses went with her husband. Operative removal of the uterine fibroid had no effect. Gradually the number of her imaginary children grew to ten and more. The husband was declared to have become a millionaire through the money the doctors had given him for the opportunity of experimentation. She again learned to smooth over her ideas, but after a few months at home she returned as daughter of the Queen of Russia, with elaborate systematizations. She is a good worker in the sewing room, discusses her situation pleasantly and coherently if tactfully treated, still asserts that she is pregnant—a typical paranoid compensatory wish-fulfillment, gradually attained after a period of dream experiences uncorrected in her waking life.

From these grave cases many transitions lead over to those in which we see mainly episodic tantrums break through, usually in the form of conflicts, or of wish fulfillments with varying elaboration within the situation, and a number of more or less characteristic traits that mark the disorders as reactions to complexes, faulty and perverted attempts to meet more or less real difficulties and breaks of integration. The chief point is that concrete difficulties and states of tension can be demonstrated and that the course shows a distinct relation between balancing material and further evolution.

We should have to refer further to those cases in which as a rule definite strings of developments appear in a person either originally with difficulties of make-up, or transformed or made unresistive through progressively deteriorating habits of adjustment.

Another case is that of a bright young woman who went through nine months of a catatonic attack with a delirious episode, negativism,

refusal of food, retention of saliva, stereotyped attitudes, echolalia, grimacing, etc.; improved at the end of nine months, went home almost well, but in a few months became sleepless, harped bitterly about fantastic ill-treatment at the first hospital to which she had been taken, again refused food, complained of pain in the left shoulder, improved slowly at the hospital, was again better and might have gone home if the circumstances had permitted; then got worse again and says now, she has no touch with her real environment; her mind dwells on the old story of ill-treatment and she hears remarks on it; she is made to suffer here for another woman; she had refused work for months out of a feeling of aversion and disappointment when transferred to a ward for more chronic patients, but even here was again found at work and much more affable after a review of her situation. Yet this woman is, through fate and the development of her make-up, relegated to the ranks of disappointments of treatment.

Good informants saw nothing peculiar in her and called her efficient, practical, not dreamy. On closer inquiry she was described as very scrupulous about the feelings of others, and equally sensitive to slights, proud of her appearance and reputation, so prudish that she would never undress in the presence of her sister and unusually sensitive about references to sexual matters. She had times when she wanted to be alone and felt nervous and complained of weakness and stomach troubles. Her father had died insane; and a brother (one of five) had had epileptic insanity. The patient herself complains that she *never* was practical; she was a great reader and her sister tells us of a huge scrap book of poetry. From 1905-07 she stayed away from confession. A young woman against whom her sister had warned her, introduced a man to her concerning whom she first spoke with aversion, but who evidently fascinated her under a decided conflict. She was much mortified because she found out that he was a divorced protestant and he left her after having hurt her shoulder by lifting her up playfully, and after borrowing some money. This was followed by another blunder and conflict; although deeply averse to divorces, the patient helped a young woman get evidence for a divorce from a supposed bigamist, worked all her spare time and found out in the end that there had not been any marriage at all. This was followed by her moving to another city; there the conflict preyed on her and a depression came on which rapidly led to a catatonic climax, without the slightest attempt at rapport on the part of the patient or physician, until she was transferred to another hospital and finally discharged after an illness of nine months. The pain in the shoulder which had recurred when she came to us disappeared after an electrical examination of the really existing slight atrophy. The details of the development and the reminiscences from the first hospital were then gone into, but evidently not traced completely to a balance or to the fundamental sore points. The circumstances of her family and the crowding of the hospital necessitated compromises to which she reacted unfavorably several times after a certain level was reached; she relapsed and got out of touch with her real environment. She says distinctly—'my mind is always away from here.' Yet the adjustments in changing the mere ruminations into open discussion and giving space for direct and concrete interests has an unmistakable influence on the patient.

As a purely transitory disorder but with a host of 'pseudo-spontaneous experiences' and feelings of being *made* to do things, I should like to refer to a girl of 23 who made as it were a spontaneous recovery under mere quieting treatment and simple straightening out of the puzzle. Rather inefficient, unable to adjust her work and interests

smoothly, never holding her places as servant long, often sulky, shut-in, and with few friends, without any heredity and denying sexual abuse, the patient had become more self-absorbed about New Year, 1904; in March she had a short attack of articular rheumatism. After it she was even more self-absorbed, was sent on a vacation, after that began to offset the actual condition by claiming that her work was better, that the other girls made trouble and were jealous; at other times she remarked she had something terrible on her mind. An ultimatum about her inefficiency in November finally made her pass into a state in which she was odd, untidy, adding 'thank you' to everything she said, even to her sister. When a man came into the house she ran away, got on her knees and prayed. The next morning, after a few moments of normal manner, she had to be taken to the hospital and disclosed a whole complex of imaginative material, that a young man used to come to her room at night, that she felt she was pregnant, and then stopped feeling the movements; she must have conceived from a spirit, etc. She was run down, admitted dreaming much. Examination spoke against all possibility of her claims. But she described how the feeling developed, how her head felt queer, how she felt her head move, and her hands as if practicing the piano, but without *hearing* orders or voices or music. Even her work was at times forced on her in this pseudo-spontaneous way. One time she lost her speech for a few minutes although she could work her tongue. She gained rapidly, gradually became less apathetic and described the experience with full appreciation and employed herself. She was discharged restored in three months. It is not difficult to see in the whole experience a wish-conflict, traced to a man who lived in the house in the spring but who hardly ever spoke to her. The constitutional make-up with conflicts in her ordinary reactions, a certain amount of ill-health, frequent dreams and finally a tantrum, without the systematic amnesia of hysteria, gradually resolved again without complete ventilation of the entire mechanism. The observation dates from 1905. No catamnestic facts obtainable.

These sketches must suffice. They should show that they deal with developments far from being inconceivable as chains of faulty mental adjustment and far from demanding artificial explanations by specially invented poisons, and a clamoring for invented "things back of it all," if at least we acknowledge the long time and mass of doings and their kind.

These conditions have been grouped together by Kraepelin under the term Dementia Præcox, embracing derangements which very often tend to end in peculiar defect conditions, ranging from the not infrequent cases of simple disappointment of parental hopes by apparently promising individuals who fail to make their mark, to cases with rather characteristic mental upsets and characteristic usually progressive apathetic dementia.

These cases do indeed make a group worth distinguishing as a nosological entity and they offer certain common and characteristic traits always carrying a warning that the tendency is towards deterioration. The unfortunate feature of Kraepelin's view is that this possibility or great probability is made to appear as a great dogmatic certainty, dictated by

merely suggestive but not causally correlated signs which are seen in the end-stages and appear also in the very beginnings, as mere empirical ear-marks, merely classified as disorders of emotion and volition, hypothetically due to toxines and brain-lesions, but not reduced to any chain of cause and effect.

The picture as a *whole* makes the *diagnosis*. There are no decisively pathognomonic facts. The deterioration gives the disorder its name; but it need not always be realized; the essence of the process is a hypothetical toxic influence or disorder of metabolism — entirely hypothetical — with definite brain-lesions—also vague and not explained. What he deems essential comes out most clearly in his differential diagnoses, where we find the enumeration: emotional apathy, and specific disorders of will: negativism (mutism, refusal of food, etc.), automatism (catalepsy, echopraxia and echolalia) and mannerisms (grimaces, oddity, stereotypy, verbigeration), silliness, unaccountable and odd acts, etc.

This description, to a great extent taken over from Hecker and Kahlbaum, figured originally under the 'degenerative' disorders. There it would be a mere dispute about words to debate whether or not the early symptoms were evidence of the disease or not. The fact that these early signs *need* not lead to more trouble might still be compatible with calling them degenerative, and the mental factors *might* be admitted to play a more or less active rôle in the development of those cases which progressed further. But in 1896, Kraepelin, taking general paralysis as the paradigm of psychiatry — each disease having a definite etiology, definite course and outcome —included a much wider range of cases in the original group, viz., practically all cases of the simple psychoses which tend not to recover or are apt to deteriorate in the end; and he explained them all as disorders of autointoxication with a special assumed brain-disorder. Then the question might have arisen: If we deal with a toxic state where does it become established and when; and what would we have to modify to prevent it? What rôle do the biological reactions play which represent the early symptoms?

Kraepelin purposely declines any idea that special antecedents in the life of the patient are worth considering as causal or even as aggravating dynamic factors. (About' 20% would, according to him, show some early premonitory signs like seclusiveness, oddity, excessive religious devotion, moral instability, but trusting his *deus ex machina* he sees in this mere evidence of a very early setting-in of the so-called 'disease itself.' As a matter of fact the cases in which early symptoms *are* found are much more numerous than 20%; as I claimed in 1903, a very *large* number of these cases show what Hoch has

lately called a shut-in personality, specially exposed to inner friction,—a percentage of actual demonstration about as great as that of actual demonstration of evidence or suspicion of syphilis in general paresis. Kraepelin, however, underrated these facts and by absorbing many doubtful and poorly analyzed cases in his group came to suggest that this disease might befall any one, and that it was an autonomous brain-disease.

Even in the hands of the originators of this new large entity embracing all the cases passing into apathetic deterioration and many others that at least tended to deteriorate, the definition of the term is evidently much more fluctuating than the uncompromising theory would suggest. In Heidelberg it has fluctuated from 8% to 52%, and now back to 18% of all the admissions. In the Munich clinic the optimistic tendency is still more on the increase and fewer cases are dubbed dementia præcox on the female side than on the male side. The inevitable conclusion is that between dementia præcox and manic-depressive insanity and simple psychopathy there is an uncertain territory which refuses a categorical arrangement in the easy and simple dogmatic terms that 'some disorders *must* be a deteriorative brain-disease because they early present certain signs also seen in actually accomplished deteriorations' and the claim that it would be futile to make an effort to analyze the data as a whole in terms of cause and effect.

To this empirical and formal conception I have opposed for a number of years a conception which aimed to be less dogmatic and more likely to be conducive to the determination of the facts actually present in the cases in terms of an experiment of nature, in terms of determinable initial constellations, reactions with probabilities rather than fixed laws of termination; in terms of dynamic and possibly modifiable factors and in terms of natural non-dogmatic developments, to quite an extent measurable in advance by the facts at hand in the case and not merely by the intermediary of a dogmatic fate-like noumenon or largely hypothetical construction.

As dynamic factors in these developments there stand out certain activities and states of disturbed balance and regulations which have far-reaching effects upon the mental adjustments themselves, and *incidentally* upon the organic understructure of the personality.

We have so far failed to find any tangible poisons and infections as in any way essential in the process. The extent to which regulative substances akin to hormones may play a rôle and figure as non-mental short-cuts of reaction is a problem for the future to decide. Berkley's claim of hyperthyroidism is not very convincing to one familiar with a goitre-district and large numbers of thyroid affections; Kraepelin's suggestion

that the poison may have some relation to the sexual functions merely flirts with the truth and is so vague as to demand consideration only if actual facts can be adduced and other facts should fail.

On the other hand, we find in evidence factors which are apt to shape or undo a life—specific defects or disorders of balance, with special tendencies and *habitual* ways of bungling and substitutions and a special make-up which is liable to breakdown in specific manners.

In my first formulation of the situation in Toronto (*British Medical Journ.*, Sept., 1906), I started from the paradigm of *complete action* as the function which gets more and more disorganized by first trivial and harmless *subterfuges* or *substitutions* which, in some individuals, lead further, become harmful and uncontrollable, tend to assume types of definite anomalous mechanisms, unintelligible and crazy if viewed apart, but more or less intelligible as a string of actions substituting, and often missing, an efficient adjustment to concrete and actual difficulties.

These substitutions constitute the symptomatology and chains of events which we have found in the cases described and which I need not rehearse and analyze before you owing to the shortness of the time and because the facts in the cases described are more trustworthy than verbal formulas. Suffice it to say that we meet neurasthenia, hysteria or psychasthenia-like substitutions, or mere dilapidations of interests or states of conflict or depressions of a morose, *topical* character—usually with one or more initial tantrums of the character described,—and delusional developments either with episodes of ruminations and giggling and the like, which may absorb more and more of the patient's actual life, or catatonic developments, or paranoia-like delusional states, all with a number of ear-marks: in the main freedom of the hysterical haziness and tendency to systematic amnesia, but evidence of conflicts of reaction, of blocking, of peculiar automatic interferences, *i. e.*, evident disorders of the highest integrations, and fantastic ruts especially in the sexual or religious spheres and their symbolic elaborations, with very frequent dissociation of the personality and pseudo-spontaneous experiences; further, a growing divorce from the concrete environment, a deterioration of interests and perversion of impulses and actions.

I must pass over the systematic attempt to account for the various symptomatic mental and non-mental developments, as I am convinced that the complete account of the cases with their concrete settings and developments is bound to be the best and only safe basis for deductions, although I realize that the brevity of the sketches left untouched many legitimate

queries, especially those about the non-mental or so-called physical components of the reactions, and the possibility of accounting for them with habitual substitutions and habit-conflicts. I only wish to refer to the catatonic reactions which are especially often mentioned, not only as being oftenest connected with certain brain lesions, but as being unexplainable from the psychological side. The catatonic reaction is by no means so far from yielding to a psycho-biological interpretation. It is a breaking down of normal conduct and adaptation too closely related to what is seen in hypnotic states and in mystic fancies; too directly like stages in religious symbolism and feelings of submission to influences by mystic powers to be compared with what happens in organic psychoses. In general paralysis and arteriosclerosis and senile deterioration, it is not the *synthesis* into a personal integration that is *most* lacking, but the *material* used in the synthesis is decreasing, through lack of memory, judgment and the range of capacity, without any distinct following of the lines of functional cleavage in the process of disorganization. In dementia præcox the dissociations follow the lines of functional and topical complexes. The very frequency with which especially catatonic reactions appear outside of the actual deteriorations, though preferably in dementia præcox, would corroborate their interpretation as a specific functional reaction type possibly founded on a phylogenetically very old reaction partly of protection or partly of mystic surrender. If they are apt to appear occasionally in organic psychoses, the same holds for manic-depressive and other more essentially psychogenetic reactions. It is, however, certainly significant that catatonic disorders are *most* apt to accompany the *traumatic* forms of organic disorders, such as also produce hysterias and other after-effects most likely connected with a functional shock. In the simple *dilapidation* and the paranoic developments, the psychological staging is too much in keeping with the situation and the harmonious evolution on prevailing premises, to create serious doubt against an essentially functional interpretation of the evolution and, also, of the lesions which may be found.

Sizing up the disorder in terms of a break in the working of conflicts, of balance rather than in terms of an autonomous disease of the brain, will stand and fall with the extent to which the initial data allow us to predict the course of nature's experiment, a point concerning which only the publication of casuistic material will give sufficient proof. Our work with these principles warrants the conclusion that while general paralysis is *relatively* incalculable in the *details* of its course, and certainly remarkably *in*dependent of mental determinants, the fluctuations observable in dementia præcox are decidedly too of-

ten accounted for by renewed up-sets and tangles and irritation of idiosyncracies, and that the prognosis of the ultimate tendency is remarkably often foretold, so that of the cases interpreted as actual deteriorations but few surprise us with a recovery, and those that *do* recover are as a rule specified at the outset as cases merely akin to this group worth naming by the end-stage, but with varying amounts of balancing material. Such a disorder is, to be sure, as little open to *absolute* prediction as life's vicissitudes, and a continued test of estimates of events in the light of ultimate results gives one a certain reserve and modesty; but, with it all, the conviction grows that the factors depended on in the estimate of the make-up and in the ratio of the reaction and balancing material, are really *factors at work*, and leave less and less space to a craving for what is 'back of it,' instead of attention to what is the 'go.'

Where a break or morbid reaction has once set in, it is very difficult to bring relief directly. The fundamental shutting in and the whole mechanism enables the preoccupations to live themselves out and to exclude interference. Automatic resistance against the most natural impulses frustrate even the occasional pathetic spontaneous appeals of the patient for help. The best procedure is to tide over the acute tangle with as much tact and ease as possible, to promote relaxation, and to relieve the situation wherever that can be done, bearing in mind the facts obtained referring to the upsetting factors, the probable complex-constellations and prevailing physical disorders. As soon as the patients feel that they meet with help instead of an argumentative and corrective attitude they can be led considerably when the time comes or where the difficulty has not led to complete blocking. Then a positive re-education in the form of habit-training and of readjustment has to set in. It is obvious that experience brings a certain divination and that individual capacity plays a decided rôle in the straightening out of the difficulties, both during the tangles, and in ultimately marshalling the forces to a more practical unity and level again; it is also obvious that we cannot be very optimistic in most cases, as little as when we try to win over our less unbalanced neighbors to a better mode of thought, belief and conduct and behavior.

We owe to our European guests, Professor Freud and Dr. Jung, the demonstration that what is at work in the centre of the stage is a complex or group of complexes consisting of insufficiently balanced experiences in various ways modified by symbolism. Their ingenious interpretations have made possible a remarkable clearing up of many otherwise perplexing products of morbid fancy, in ways the discussion of which, no doubt, I had better leave to their lectures.

Yet, if I interpret their accounts correctly the reason why only few persons create these complexes and fewer yet develop them to a disastrous form and often to a deterioration, is mainly left to heredity or finally to toxines, whereas I would prefer to adhere to my attempt to define the responsible factors as far as possible in terms of prophylactic suggestiveness, in terms of untimely evocation of instincts and longings (acting as fatally as premature destruction of naïveté), and ensuing *habit-conflicts* with their effects on the balance of the person, and on the sum total of mental metabolism and actual doings and on the capacity for regulations in emergencies. In some cases the *habit-disorders* preponderate in the side-tracking and the curbing of leading interests and creation of disastrous substitutions; in others, definite complexes play a special rôle and as a rule the sizing up of the disposition must consider both factors. In practically all cases the scope and funds of mental deviation form a consistent evolution and offer the safest material for prognosis and practical handling.

For all I can see the main obstacle to a wider acceptance of a functional theory in terms of habit and complex conflicts and definite responses thereto, is on the one hand the habitual or intentional lack of the necessary penetration into the life of the patient and family, and on the other hand, the readiness of the physician to turn to set interpretations and to reiterate authoritative statements with a certain pedagogical self-sufficiency. I refer especially to the traditional rut shown by physicians when they have to meet the question of habit-disorders, such as masturbation, which invariably leads to reasoning in a circle by calling the disorder a sympton of a disease and evading the possible rôle in additional abnormal developments instead of to a frank inquiry into the facts and difficulties in the case. Further, there is perhaps also a more or less legitimate aversion to any extreme dogma, using too exclusively the sexual origin or the weight of complexes, and special displacement mechanisms, and an aversion to certain other 'atomistic' types of psychopatholgy, and especially also the fact that so many spontaneous recoveries occur and also many failures under almost any procedure.

The most serious cause for relapses into opposition to psychogenetic interpretations is the blind acceptance of any anatomical findings as definite evidence of an autonomous disease, after the paradigma of general paralysis. And to this point I wish to give a brief discussion.

The lesions found by Sioli and others are very different from those of general paralysis in their nature *and* as to autonomy of origin. They are most akin to fatty involution of the brain tissues, probably as incidental to the disorder of function as is

the brown atrophy of the heart, the fatty degeneration of muscles or of the liver. The one disease in which disorders similar to those in dementia præcox, and even more marked, have been seen, is Huntington's chorea which is a striking instance of familial insufficiency of the nervous system, and hardly a product of a toxic disorder. The occasional late recoveries of apparently demented patients and the peculiar clearing up of some cases during intercurrent diseases—in which the most *vital* instincts of self-preservation and of complex-free family interest are brought out again—would certainly make one doubtful about the "profound deteriorations of the cortex" being on an autonomous basis as in general paralysis. Until we know much more about the amœboid neuroglia and the protagon degeneration seen in dementia peæcox *and* in Huntington's chorea and probably elsewhere, we certainly do well to leave open the question whether a disorder of anabolism and catabolism incidental to the prolonged and often profoundly vitiated attitudes and defects of balance is not sufficient to explain the findings (which are possibly as incidental to special chronic disorders of function as the finding that Dr. Hodge has established in acute fatigue states), or to what extent they are perhaps short-circuits; that they are incidental to a broad frame, seems unshakable and the more we teach the physician to think in terms of what is demonstrable in the case, the better for him and for the patient and for prophylaxis and for the formulation of further problems of investigation.

The lesion in general paralysis is of a totally different kind, depending on a previous infection with syphilis and forming a peculiar infiltration of the brain vessels, similar to what happens in the African sleeping sickness, but accompanied by additional degenerative processes in the brain tissue. This exogenous disturbance leads to death within a limited number of years, and accounts for certain fundamental symptoms of dementia of a kind quite different from that in dementia præcox. In addition to that, there are, however, symptoms not common to all cases, such as the development of exaltations or depressions or delusional states, sometimes following certain traits of dementia præcox. These superadded psychotic symptoms have been attributed to different localizations or distribution of the characteristic lesions. A careful inquiry into this question on the material in the literature and our own observations of focal general paralysis shows, however, that the focal lesions may give aphasic attacks or neurological disturbances, and occasionally precipitate epileptiform reactions with amnesic phases, fugues, and states of bewilderment; but the psychotic symptom-complexes occur without any regularity. In one case of Alzheimer a dementia præcox-like disorder of

paranoid hallucinatory developments was connected with special affection of the left parieto-temporal region, but the patient had had an earlier attack six years before the suspicion of general paralysis arose. Such a case as this and a number of others suggest strongly that these *usually* psychogenetic disturbances depend more definitely on the previous mental make-up, even in the general paralytic, very much as has lately been admitted by Bonhoeffer and Homburger in certain alcoholic and exhaustive disorders—Homburger being a pupil of the Heidelberg school but under the influence of the master of functional pathology, Krehl.

In view of these considerations it is unintelligible that analogy with general paralysis could be strongly enough founded, to excuse a recent writer on the insane in Massachusetts who urges or sanctions on this ground a plea of medical ignorance with the following remarkable conclusion: "Until we have learned more by continuous study of the causation and pathology of dementia præcox, curative measures will be most fruitfully employed in the manic-depressive and toxic cases, to increase the percentage of recoveries and diminish the number of deaths."

The comfort of working under the cover of fatalistic and analyzed conceptions of heredity, degeneracy and mysterious brain-diseases—and the relief from responsibility concerning a real understanding of the conditions at hand, and concerning the avoidance of preventable developments—is a powerful and unconsciously cherished *protection*, very rudely disturbed by these conceptions which make the physician partly responsible for the plain and manageable facts. I deny that fatalism is inevitable, without admitting that my conception should imply unwarranted optimism. It is merely a return to the facts at hand which will prepare us all the better for the actual work, and pave the way towards prophylaxis where something can be done. The position is, however, equally important in the utilization in psychological teaching. There probably is a certain comfort in arranging the courses within a narrow range of laboratory problems. Unfortunately, that does not always train the student's sense in using the foot-rule of ordinary life with any degree of accuracy or conscience, when he passes to more complex domains. Scientific accuracy in one field does not guarantee a critical attitude in the fields of nature's experiments which are complex and cover larger spans. If we make the student wade through a mass of rather artificial psychological laboratory work, and on the other hand, equally artificial philosophical puzzles, we would leave him in the end without help and training to meet some essentials in life. Even a non-technical knowledge of the facts in some mental

patients is bound to widen the horizon and would to my mind be an intrinsic part of any course or programme of psychology (as good as, or better than, an abstract course on mind and body). Without its concrete lessons many events appear like puzzles and are unduly treated as such. This stands out glaringly in a recent book on psychotherapy which makes the reader divide the attitude of appreciation and the attitude of physical explanation without helping him to unite them again; which contrasts the subjective and objective and the purposive and the causal view without bringing them to the common denominator of experience again; which urges him to split psychiatry and psychotherapy—and therein fails to be helpful in the very task of sane instruction, namely, that of integrating disconnected facts into sane 'organized common-sense.' Familiarity with the concrete events in nature's experiments would reduce the longing for these artifacts.

I have on purpose avoided entering upon the details of many excellent modern trends of psychological investigation in our field. I wanted to make a plea for the broader *frame* of things. This frame must be grasped with an understanding of the broader elements in the disorders with which we deal. Within this frame the details get their perspectives. In the theory-ridden physician and in the ultra exact psychological laboratory worker, I should like to awaken the natural instinct of curiosity concerning the keenly interesting broader biological settings brought out by the mental disorders and destinies discussed. I should like to make all feel the sanctity and paramount interest of the concrete cases. I cannot resist recalling what is so well expressed in the recent Presidential address of the great physicist, J. J. Thomson, in his appeal to the mathematician to avail himself of the power of the concrete. He says: "Most of us need to tackle some definite difficulty before our minds develop whatever power they may possess;" and we cannot deny that the field of habit conflicts and of far-reaching and complex emotions and longings gets its most wonderful representatives in disease. Ribot opens his last study with the remark: "Le meilleur procédé d'expérimentation en psychologie, à mon avis, est la maladie aves ses désordres." Diseases are the most crucial experiments in man. Here the momentous things occur in a way which might well supplement the man-made experiments of our laboratories and suggest problems in a way which really go at the causal relationships vital to the student, vital to any layman who wants to know what psychology is and does, and vital to the physician who wants to help also where help would rarely come without him, and may even be too late with him, as long as we fail to make sure of prophylaxis.

We are, I believe, justified in directing our attention to the factors which we *see* at *work* in the life-history of the cases of so-called dementia præcox. We are justified in emphasizing the process of a crowding out of normal reactions, of a substitution of inferior reactions, some of which determine a cleavage along distinctly psychobiological lines incompatible with reintegration. Psychobiological analysis and reconstruction furnish us the essential material, and progress is to be expected from a frank and unprejudiced weighing and use of this material including its non-mental components rather than from the stereotyped lesion-pathology and the dogmatic nosological principles when they become intolerant.

I could not have had a more delightful opportunity to present a discussion of the essential facts in favor of a dynamic conception of dementia præcox than this occasion as I realize that my development has to no small extent been influenced by the spirit at Clark University, its genetic attitude and the liberality in admitting the facts for investigation whether they seemed to fit preconceived plans or not, and its strong faith in the selective capacity of interest and in an unprejudiced inquiry with or without laboratory methods, but always with an interest in the conditions under which reactions develop.

THE PAST DECADE IN EXPERIMENTAL PSYCHOLOGY[1]

By E. B. Titchener

I am to speak in this hour of the course and progress of experimental psychology during the past ten years. The psychological laboratory has, as you know, had but a short history; and the modern psychologist counts in decades, as the historian of human thought counts in centuries. It is, I hope, not out of place to remind you that even the century is an artificial unit; when we think of the philosophy of the eighteenth century we certainly include Locke in our list of writers, although Locke died in 1704; when we think of the psychology of the nineteenth century we certainly do not include Fechner, although Fechner came after Herbart. The century, none the less, serves a useful purpose, because a hundred years, three human generations, are as a rule sufficient for the testing of an idea, for its establishment or its final rejection. Of our own unit, the decade, we can say no more than that, in the precocious development of the later-born sciences, it is at least a period long enough to warrant an inventory or stock-taking, from which we may gauge the trend of interest in the immediate past, formulate problems for the present, and possibly infer the direction of inquiry in the immediate future. The ten years whose psychological activity I am to review have, however, a special claim upon the historian. Beginning in the nineteenth, they lead us at once across the line of secular division into the twentieth century; they embrace the culmination of the one and the first effort of the other; and I cannot resist the belief that all the sciences, old and young alike, approached this twentieth century with a certain self-conscious expectancy which, however difficult to appreciate to-day, will presently be seen to have exercised a marked influence upon the intellectual movement of the time. It is, then, if I read the signs aright, a period of unusual interest and of especial scientific significance that has formed the second decade in the life-history of Clark University.

Nevertheless, as I approach the topic of this lecture, what is

[1] Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

uppermost in my mind is a sense of irreparable loss. When the cable brought the bare news, last February, that Ebbinghaus was dead, just a month after the celebration of his fifty-ninth birthday, the feeling that took precedence even of personal sorrow was the wonder what experimental psychology would do without him. You are all familiar with Ebbinghaus' work; to some of you, as to myself, his death has meant the loss of a friend: those who had not known him, but had looked forward to his promised address from this platform, have missed an experience that should have remained a lifelong memory. I shall not here attempt an eulogy: that is unnecessary. But I must remind you that Ebbinghaus' qualities were precisely those that, in its present stage of evolution, experimental psychology seems most to need. What characterized him was, first, an instinctive grasp of the scientific aspect of a problem,—scientific as distinct from philosophical, in all the protean meanings of that latter term; secondly, a perfect clarity of thought and of language, the expression of thought; and thirdly, an easy mastery of the facts. I say mastery; but the truth requires a stronger word. There was about Ebbinghaus a sort of masterfulness; he never did violence to the facts, but he marshalled them; he made them stand and deliver; he took from them, as of right, all that they contained; and with the tribute thus exacted he built up his theories and his system. This, I say, was the example that we needed, in a time when psychology still appears helplessly entangled with theory of knowledge; when the standard of scientific writing, so far as literary style is concerned, is deplorably low; when theory is impatient of fact, and the facts themselves are scattered and inco-ordinate. I believe, indeed, that Ebbinghaus' *Grundzüge*, already in its incomplete form a centre of widespread influence, was destined to a place of leadership; I have sometimes thought that, with allowance made for changed conditions, it might prove as important to experimental psychology even as Wundt's *Physiologische Psychologie* or Brentano's *Psychologie vom empirischen Standpunkte*. But Ebbinghaus is gone; and with his passing our science has sustained the most grievous loss that it has been called upon to bear since Delbœuf undertook that fatal journey to Munich thirteen years ago.

These brief remarks, inadequate as you must feel them to be, will at any rate in their spirit and intention command your assent. I pass now to another preliminary matter, upon which agreement is hardly to be expected. For agreement implies, in the first instance, a common point of view; and my own standpoint, which is that of pure science, or the desire for knowledge without regard to utility, is in all likelihood shared

only by a small minority of this audience. Moreover, agreement within the domain of pure science presupposes a certain measure of progress, a platform of assured results; and to that point, perhaps, experimental psychology has not yet attained. Nevertheless, while I anticipate that you will reject my conclusion, I trust that you will also remember the general attitude and point of view from which it is derived.

If, then, one were asked to sum up, in a sentence, the trend of psychology during the past ten years, one's reply would be: Psychology has leaned, very definitely, towards application. And if the questioner were thereupon to look for proof of this statement, he would find it confirmed not only by the range and variety of current practical work, but also and more particularly by the incursion into the field of practice of men whose training and previous interests might naturally have held them aloof. I shall not try to indicate the surprises that this movement towards application had in store for the theoretical psychologist; still less shall I try to set before you with any fullness the evidence for the strength and universality of the movement itself; I mention only a few typical facts. In 1903 Meumann opened the pages of his *Archiv. f. d. ges. Psychologie* to studies in applied psychology; and in 1905 Wundt alleges the preponderance of these studies as a principal reason for the foundation of the *Psychol. Studien*. In the same year 1903, Stern began the publication of his *Beiträge z. Psychol. d. Aussage*, which by 1907 had developed into the *Zeits. f. angewandte Psychol. und psychol. Sammelforschung*. Meinong writes in 1904 that experimental psychology, now brought into touch with the needs of practical life, is on the way to become a popular science; and expresses the hope that the contact of theory and practice may grow constantly closer. In 1906 Jung published the first volume of his *Diagnostische Assoziationsstudien*. In 1907 Meumann gave us two volumes of lectures on experimental pedagogics, with the promise of a system to follow. In 1908 Binet formally devotes his *Année psychologique* to the cause of practice; psychology is to be laid out and aligned with reference to practical and social questions. The present year has seen the publication, in English, of two popular works—Münsterberg's *Psychotherapy* and Watt's *Economy and Training of Memory*—which by diversity of aim and content as well as by form and style bear witness to the scope of application. I have mentioned such things as occurred to me: the more omissions you remark, the more securely will my thesis be established.

Now we have all heard it said—and said in connection with this practical tendency of recent work—that experimental psychology cannot hold its men; that its problems are not large enough to satisfy our intellectual demands; that the ex-

perimental psychologist will inevitably turn, sooner or later, to æsthetics or theory of knowledge, to physiology or general biology, to education or therapeutics. There is, in fact, some leaven of truth in that statement, and there is much untruth. Remember that no experimental science can hold its men beyond a certain term of life; remember that a large proportion of our students come late into the laboratory, and that they bring no habit of experimentation with them; and remember also the natural indolence of mankind, and how much easier it is to write an essay than to plan and carry through a series of experiments. But notice, on the other hand, that the experimental psychologist oftentimes has no choice allowed him; he may even hold his office under a foreign title; he is called to represent, in the economy of the university, a group of more or less closely related disciplines; and while no man to-day would claim for himself the title of physicist, it is not thought amiss that a philosopher or an educator should make a by-work of experimental psychology. All these things must be taken into account,—though I doubt if any one of them goes to the heart of the matter. The essential point is, surely, this: that many men, who are not by temperament psychologists, pass through the psychological laboratory on the way to their proper goal. Some are attracted by curiosity, by the mere charm of novelty; some desire in more serious mood to see and to understand. For all, psychology lies, convenient and accessible, at the cross-roads of human knowledge; and we who dwell there can only be grateful to those who for a season share our labors, while we hope, as soon as may be, to bid the casual visitor godspeed.

Under these conditions, the diversion into practical channels of energy which would otherwise have been expended in the service of the laboratory must be regarded as a definite loss to pure science; and it is from the standpoint of pure science that I am now speaking. You will reply, perhaps, that there are compensating advantages. For those who apply psychology testify, in the act, to the soundness and relative maturity of psychological ideas. And if we may judge by the experience of the older sciences, of physics and physiology, the trial of these ideas in practice will react upon the ideas themselves; application will discover new problems, which must be referred back for solution to the experimentalist. I recognize the advantages; but I do not think that they offset the loss. Every one, for instance, who has followed the history of science, knows that successful application is but a very imperfect measure of the validity of a theory; material improvement may go astonishingly far under the guidance of some scientific hypothesis which later generations

roundly pronounce erroneous, and which shows, indeed, but a glimmer of the later found truth. Pragmatism, as it was at first interpreted by its critics, and as it seems at first to have been intended by some, at least, of its propounders, could never pass muster with men of science, who see how well ideas may work upon how precarious a basis of fact. But, if we leave generalities aside, and consider the actual status of experimental psychology, we are still forced to the conclusion that this first argument overshoots the mark; it ranks as final achievement what is, for the most part, no more than tentative suggestion. And as for that other argument, of give and take, action and reaction between theory and practice, I confess that I am a little tired of it. Some day, if analogy may be trusted, it will hold; to-day it is but the expression of a pious hope. For application, if I read the documents fairly, has proceeded in two principal ways: it has borrowed and transformed some approved psychological method, or it has adopted and popularized some assured psychological result. I have nothing but respect for the men who by ingenuity and hard work make so much out of so little; but I cannot believe that either of those procedures will, by reaction, bring any considerable gain to science.

These, however, are merely counter-arguments, rebuttals; and it behooves a minority to be aggressive. Let me add, then, a positive statement. So far is experimental psychology from any general readiness to furnish ideas for application, that applied psychology has been obliged to think out ideas for itself; and so far is applied psychology from reliance upon the parent discipline, that some of its most widely used and most strongly emphasized ideas contravene established scientific principles. The notion of a quasi-mechanical dissociation, for example, or various modern forms of the doctrine of the subconscious,—these ideas are both foreign to the spirit and inadequate to the status of experimental psychology; they are stumbling-blocks in the path of scientific enquiry, obstacles that it will take time and labor to overcome. Not only, therefore, has the movement of the last few years, by its withdrawal of men and its substitution of practical for theoretical interests, brought with it a loss to science; but it has also, in the manner indicated, placed positive hindrances in the way of scientific advance.

That is the situation, as it appears from my point of view: others will see it differently. It is, in any case, a situation that must be accepted; and we have now to consider how, because or in spite of it, experimental psychology, the theoretical psychology of the laboratory, has fared during the past ten years.

I wish that I could proceed systematically. But, as we have already seen, psychology is not ripe for systematization. Let that be said emphatically:—and when you hear criticism of the claims advanced or the promises made by experimental psychology, remember, in all justice, that it has been said emphatically, and be at the pains to discover whether the claims and the promises come from psychology itself or from some less responsible enthusiasm. We must, in fact, proceed topically, though we shall naturally begin with the three fundamental topics of sensation, affection and attention.

In the sphere (1) of *sensation*, the most striking fact to record is the revival and extension of Fechnerian psychophysics. We have but to exceed our time-limit by one year, and we may start out with Martin and Müller's work upon the differential sensitivity (1899). Then follow Foucault's *Psychophysique* in 1901, Wundt's fifth edition in 1902, Lipps' *Grundriss* in 1903, Müller's *Gesichtspunkte und Tatsachen* in 1904, Aliotta's *La misura in psicologia sperimentale* and my own *Experimental Psychology* ii. in 1905, Lehmann's *Psychologische Methodik* in 1906, Wundt's sixth edition and Urban's *Statistical Methods* in 1908—to say nothing of a number of authors (Ament, Fröbes, Mosch, Holt, Laub and others) who have published single papers of considerable theoretical interest. Truly, there is hope for a generation which, in the midst of practical activities and despite the clamor for results, can find the time and the men to pursue in this persistent way the study of scientific method! And the outcome? The outcome is that, on the level of ordinary quantitative work, we now know enough to check our methods; we can prove our sums, as the children say; our figures have a definite scientific meaning and a general scientific setting. There will still be differences of opinion; there will still, no doubt, be controversies; force of habit would see to that, for some years to come, even if there were no questions of fact that remained unsettled. But it is a great deal, in science, to be sure of your ground, to have sifted out personal opinion and speculative guesswork from observed fact and genuine problem; and I think we may say, without undue optimism, that psychophysics has now reached that stage of self-knowledge, and has thus been lifted, with no fear of relapse, to a higher plane.

This, as I said, is the most obvious advance to record in the recent psychology of sensation. You could, indeed, hardly expect that progress should be as clearly marked on the side of quality, where the mass of established fact was already very large, and where—thanks to the co-operation of the physiologists—productive work has been continuous. If, however, I were called upon to mention a single noteworthy

event, I should select the re-issue of Hering's *Lehre vom Lichtsinn*, in which the veteran author has set himself, after the lapse of a generation, to theorize afresh the whole realm of visual sensation. Another gift from physiology is the third volume of Nagel's *Handbuch*, which will henceforth be to our students what Hermann's *Handbuch* has been to ourselves. And if we feel a natural regret that the old order should change and the authorities of our youth lapse into forgetfulness, let us at the same time recognize that the change does but emphasize the debt of psychology to its sister science: a debt, moreover, that must remain unpaid so long as physiology refuses the sole return that we can make, and physiologists decline instruction in psychology. For the rest, I may roughly indicate the progress of the decade by reminding you that we have to-day no satisfactory theory of sight or hearing, of taste or smell, of pressure or temperature; the generalizations that worked so well for science a few years ago, and that still do good service in the text-books, are in the minds of those who know breaking down under the stress of newly discovered facts; and the passing of established views is only one aspect of a process which, looked at from the positive side, leads to reconstruction. But again, if I must be concrete, I can choose a single instance: the revival during the last few years—in the hands of Meumann, Becher, Murray—of experimental interest in the organic sensations. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition, in the motives to action, in the primary perception of self, in many of the complex formations that go by the general name of thought: when we remember the systematic questions that hinge upon it,—the question of the elementariness of pleasantness-unpleasantness, of the relative range of sensation and image, of what is called affective memory, and so on; we can hardly fail to see that these modest beginnings promise to fill up a great and painful gap in our psychological knowledge. It is too early to ask for results. It is something if we realize, in a fairly definite way, the difficulties of method that the experimenter must overcome before results are obtained; or rather,—it is everything that the experimental study of organic sensation has actually been attempted.

I undertook to speak of progress, not of problems. Yet I do not like to miss this opportunity of public reference to a problem which, although it has already been approached here and there, in a general way, seems to me just now to call pressingly for detailed treatment: I mean, the problem of sensible dura-

tion. In our analytical study of complex formations,—perceptions, perceptual groups, and especially total consciousnesses, —we must, I am convinced, make greater use of the temporal attributes of sensations. Is there any one who has not, time and again, been puzzled and surprised that experiences which felt so differently should come out so similarly in analytical terms? Well, experimental psychology, in its natural concern for intensity and quality, has unduly neglected the other aspects of sensation, and among them this aspect of duration. Wundt has set us a good example; he has employed duration, as an instrument of analysis, in his recent discussions of feeling and emotion; and I venture to suggest that the example is one to be followed in all departments of laboratory work.

This mention of Wundt takes us easily to our second introductory topic, the topic (2) of *affection*. Wundt, as you will remember, published in 1896 the first draught of his tridimensional theory, and in 1900 his *Bemerkungen zur Theorie der Gefühle;* Stumpf published in 1906 his paper *Ueber Gefühlsempfindungen*. These dates are significant: for they imply that the experimental psychology of feeling, which begins with Féré in 1887 and Lehmann in 1892, attains its full development in the decennium now under review. The threshold year 1899 is important here, by reason of Lehmann and Angell and Thompson, as we saw it to be important for psychophysics. Then follow, in quick succession, the experimental studies of Zoneff and Meumann, Brahn, Titchener, Gent, Bonsor, MacDougall, Orth, Boggs, Gordon, Störring, Kelchner, Hayes, Urban, Johnston, Keith, Shepard, Alechsieff, Salow, Kaestner, Nakashima, —studies that differ in method and in result as they differ also in scientific value, but that are all alike aimed directly upon an experimental control of the affective reaction. How near we have come to a stable psychology of feeling, I confess that I do not know; the smoke of controversy lingers, and it is impossible to get a clear view of the field of battle. Perhaps, for a little while, we shall none of us know; perhaps, we must await some new strategic movement to reveal the strength or the weakness of the various positions. I read, only the other day, that the attempt to build up feelings out of sensations has now been completely abandoned,—and I remembered, with some amusement, how many psychologists are still at work upon that task. No! in all such statements the wish is father to the thought, and the thought is but the wish become dogmatic. A more cautious estimate might, I think, venture upon three propositions, but could hardly go beyond them: first, that we have transcended, for good and all, that pseudo-Darwinism which, running directly counter to the intention and the accomplishment of Darwin's great work,

offers a facile teleology in place of scientific explanation, and deems the affective problem solved when it has written 'useful' for 'pleasant' and 'harmful' for 'unpleasant;' secondly, that our working hypotheses are adequate, intensively and extensively, in range and in detail, to serve as guides to experiment; and, thirdly, that the investigations so far published, inconclusive as you may judge them to be, nevertheless prove that experiment is possible. And this, you must remember, is the sole source of anxiety in an experimental discipline,—whether, when a question has been asked, the method of experiment can furnish a valid answer. So there is always, at the outset, a certain rush and hurry of research, and a certain immaturity in publication; but, the main point settled, and experiment shown to be feasible, the problem is put upon the regular waiting list, to be taken up at leisure. That is, as I see things, the present status of affective psychology. In the meantime, Wundt has made his tridimensional theory the basis of a psychology of language; and Stumpf has made his theory of a centrally excited accessory sensation the basis of a genetic psychology of the tonal and musical feelings: and while the two theories are incompatible, both of them appear to work.

I pass (3) to the psychology of *attention*, which I take to be in yet another characteristic stage of development, less advanced than the psychology of sensation, but more advanced than that of affection. Historically, such a state of affairs is but natural: for experimental work upon attention was begun by Wundt in the sixties, while the experimental study of affection dates only from the late eighties of the last century. And the bare fact that it thus occurs to one to measure progress by lapse of time is significant and encouraging, since time means nothing for science until methods have been thought out and men are trained to apply them. Nevertheless, I must not give you the idea that our knowledge of attention has increased continuously, little bit by bit, as the years have passed; that there is any historical analogy of that kind between sensation and attention. On the contrary, the doctrine of attention has shown very markedly the characteristics that certain psychologists ascribe to attention itself: it has fluctuated, risen and fallen again; wave of interest has alternated with period of indifference. Ebbinghaus wrote, in 1902, that attention is a real perplexity in psychology: "die Aufmerksamkeit ist eine rechte Verlegenheit der Psychologie;" and I think that he himself felt what he wrote; his treatment of attention is a little perfunctory, as if the composing of those sections had been a disagreeable duty. The same sentence stands in 1905; and, indeed, the whole discussion has remained practically unchanged

from the first to the second edition of the *Grundzüge*. Yet I believe that, at this latter date, one of our attention waves was gathering to a head. At any rate, the next three years saw the publication of as many systematic treatises: Pillsbury's book appeared, in French and English, in 1906 and 1908, and in 1907 came Dürr's *Die Lehre von der Aufmerksamkeit* and Roerich's *L'attention spontanée et voluntaire*. It is true that the French and German authors are largely concerned with practical issues; but they deal with attention from the side of theory also; and Pillsbury's work, the most comprehensive of the three, devotes only a few pages to educational matters. Now, these men must have found something to say: and while their teaching is, in good part, inferential, even speculative, it rests, none the less, upon a broad basis of actual observation. There are two things, in particular, that to my mind attest the progress of the past decade. The phenomena that we have been wont to refer to as the fluctuations of attention are now shown, pretty definitely, to be peripherally conditioned; I, at least, am unable to put any other interpretation upon the facts; and the psychology of attention thus loses a chapter which has always been a fruitful source of confused thinking. For there are two main uses, in current investigation, of the term attention. In the practical studies of economy of learning, and the like, attention has an energetic meaning; we hear of energy of attention, distribution of attention, quickening of attention, recovery of attention, and so forth. All these phrases are readily intelligible, since they simply give precision to the usage of everyday life; and they are also sufficient for the writers' purpose, since they designate, unequivocally, certain well-marked events or phases of the process of learning. At the same time, it must be remembered that they are employed, casually and by the way, in the course of inquiries which are not themselves directed upon an analysis of attention. The point is important, if it is a little obvious, and I pause a moment to illustrate it. In work upon the affective processes, we often refer a judgment roundly to 'association;' in work upon association itself, we often record an 'attitude' of perplexity or doubt or hesitation as a link in the chain of interconnected processes: the abstract terms 'association' and 'attitude' meet the conditions of the inquiry, and no one can quarrel with their use. But no one, either, can doubt that they presuppose a psychology; they imply a foregone psychological analysis: and what holds of them holds similarly of the term 'attention.' Now, when we turn to the second, analytical meaning of attention, we find that our concern is with the clearness or vividness of conscious contents; energetics drops out of sight. But you will see that the notion of a fluctuation of attention, of apperceptive waves, suggests the ebb and flow of

energy, so that we are constantly tempted, even when engaged upon a purely analytical task, to take attention for granted, to employ it as *datum* in the very context which we have arranged for it as problem. It is in this way that confusion has arisen, and it is on this account that we may rejoice to refer to sensation phenomena that, ever since the days of N. Lange, have been attributed to the attention.

I said that there were two things that bore witness to the progress of the decade; and the second, which I can but briefly mention, is the revival, with good promise of success, of the attempt to measure the attention, to give quantitative expression to distinguishable degrees of clearness. It is still too early to discuss this work in any summary way. The impulse to it comes, most appropriately, from the Leipsic laboratory, and Wirth's further researches will not only be awaited with eager interest, but will also, as we may hope, arouse activity in other quarters.

So much, then, for sensation, affection and attention. Our topical survey will bring us next, I imagine, to that mixed medley of formations which is included under the general term (4) *perception*. It is altogether impossible to review, in the time allotted to me, the work accomplished in this field of vast range and uncertain limits: I have tried, as I was in honor bound to try, and I have given up. You must be content with a purely subjective and arbitrary selection. I found, as a matter of fact, after refreshing my memory of some hundred experimental papers, that I had been most impressed by the work of Benussi on optical illusions, on the perception of time, on the comparison of spatial distances and temporal intervals; it seems to me that Benussi's treatment of the apprehension of form, his notion of the inadequacy of idea, his doctrine of accentual figure, and so on,—that all this is of positive value in itself, and will bring forth fruit in the future; and my judgment has remained the same, as study followed study, although I am by no means able to accept Benussi's interpretations. The Austrian school—however widely we may differ from them on systematic questions—have, indeed, made notable contributions to this chapter of psychology. For the rest, these ten years appear to have hardened and emphasized the special characteristics of American work upon the perceptual problem. Perception, in the cis-Atlantic laboratories, has been given a biological setting and perspective, and the theories of perception have been motor theories. I do not think that any good has come from the intermixture of biology; on the contrary, there has been a tendency to substitute final for efficient causes, and general considerations for exact psychophysical determination.

I am also heretical enough to think that our current motor theories are both premature and one-sided: premature because they far outrun our knowledge of the motor mechanism, and one-sided because they directly attach the motor to the receptive organs, and forget the disjunctive office of the cortex. But I do not, on this account, underestimate the achievement of Dodge and Münsterberg and Judd.

I pass from perception to the cognate subjects of (5) *recognition*, *memory* and *association*. Ebbinghaus, as I need not remind you, published his experimental study of memory in 1885; and there was no lack of experimental papers in the nineties. But consider our initial year, 1900. That year saw the issue of Müller and Pilzecker's *Exp. Beitr. z. Lehre vom Gedächtniss;* of Laura Steffens' work *Ueber die motorische Einstellung*, and Lottie Steffens' *Exp. Beitr. zur Lehre vom oekonomischen Lernen;* of Kemsies' and Netschajeff's articles on the memory of children; of Smith's *Rhythmus und Arbeit.* Our decennium began well; and it ends as it began; for it was only the other day that we received the second and concluding portion of Wreschner's elaborate memoir *Die Reproduktion und Assoziation von Vorstellungen.* In truth, this investigation of memory and association illustrates and accords with the tendency of the time. Ebbinghaus himself conceived his problem rather in a practical or psychophysical than in a psychological spirit; and, in the light of what I have already said, you will find it natural that his example should be followed,—all the more natural, indeed, since in this particular field psychophysics and practice are closely and obviously related. There are, however, in reality, three distinct problems. We may aim at a psychology of memory and recognition and association; that is, we may seek to record our experience, to trace introspectively the arrangement and course of consciousness as we remember or recognize or associate. We may aim, secondly, at a psychophysics; we may trace and measure the action of the reproductive and perseverative tendencies, evaluate the parts played by the reproductive and the recognitive factors, work out formulas of the same kind as that first formula of Ebbinghaus which represents retention as a function of time elapsed. Or we may aim, thirdly, at an applied psychology; we may lay down rules for the training and the economical use of memory. There can, now, be no doubt that the two latter aims have, in the period which I am reviewing, taken precedence of the first. But psychology has by no means been neglected: Cordes' analytical study of association appeared in 1901; Whipple's analysis of the memory image and the process of judgment in discrimination, in 1901-2; Gamble and Calkins' experiments

upon recognition and comparison in 1903; while Kuhlmann, who worked for the most part in the Clark University laboratory, has published in 1906, 1907 and 1909 a series of articles devoted especially to the introspective characterization of the memory consciousness. If to the results of these enquiries we add the indirect results of the psychophysical experiments—and indirect or secondary results have always been of great importance in experimental psychology—we have a considerable body of knowledge, even though it is not as yet either organized or complete.

I come next (6) to *action.* The reaction experiment we have had always with us, ever since the psychological laboratory came into being; it is an experiment that has been sorely abused, in word and in deed; yet so many are its possibilities, that interest in it has never flagged; and we are now beginning to see that it furnishes an invaluable instrument of psychological analysis. The decade opens with a number of technical papers: W. G. Smith in 1900 and 1903, and Judd and his collaborators in the Yale laboratory in 1905, have sought to determine the nature of the reaction movement; and Alechsieff in 1900, and Bergemann in 1905, have essayed new methods of quantitative evaluation. But this, the technical or psychophysical aspect of the experiment, has been overshadowed by the psychological. In 1905, Ach published a book entitled *Ueber die Willenstätigkeit und das Denken,*— a book which reports and discusses the employment of what the author terms, a little redundantly, the 'method of systematic experimental introspection' in a series of experiments upon simple and compound reactions. He could, indeed, hardly have chosen a more promising field. For although Kuelpe said as long ago as 1893 that "reactions are nothing else than exact types of . . . voluntary action, . . . so that their mere duration is but a small part of their psychological significance," and although Wundt has repeatedly endorsed this statement, no one before Ach had made any serious attempt to build up a psychology of volition and action upon the introspective data which the reaction experiment affords. In brief outline, Ach distinguishes a fore, mid and after period of reaction: the fore period extends from signal to stimulus, the mid or principal period from stimulus to reactive movement; the after period is a time of indefinite duration, but certainly lasting several minutes, which follows immediately upon the conclusion of the experiment. The method of systematic experimental introspection then requires that the events of the fore and mid periods be introspectively examined, as a whole, during the persistence of the perseverative tendencies in the after period. I shall have to refer, in

my second lecture,[1] to the results of Ach's investigations; it is enough now to say that those results have thrown a flood of light upon the action consciousness, and that they are happily confirmed by the outcome of other experiments, especially by the work of Watt, undertaken independently and for a different purpose. Hitherto, the chapter in our text-books devoted to the psychology of action has been a medley of evolutionary biology, physiology, and vague psychological generalization; henceforth, it will contain facts of psychological observation.

I wish that I could say as much (7) for *imagination*. But indeed, whether we begin with the elementary process, the image, or whether we go to the other extreme, and regard imagination as the general name for a group of typical formations, as a concept co-ordinate with memory, we must surely say that experimental psychology is to-day hardly over the threshold of the subject. My own belief is that we must start out with the image, and determine, for instance, whether image quality is co-extensive with sensation quality, and whether imaginal difference is adequate to sensible discrimination. More especially must we make a serious effort to resolve the paradox of the image: that it is so readily confusable with sensation, and yet so easily and certainly distinguished. Much may be hoped, I think, from the current studies of imagery, and from the incidental results of work upon thought and voluntary action. Thus we have recently found, in my own laboratory, that on one important point the popular psychology of memory and imagination inverts the facts: the image is available for memory, not because or in so far as it is stable and permanent, but precisely because it is instable and breaks down; because it admits of fusion, telescoping, substitution, and thus enables us to carry a great deal of experience in condensed and abbreviated form; while the imaginative mind, at any rate on the average level, is not a mind whose images change kaleidoscopically, producing by their instability new and still newer mental combinations, but is, on the contrary, a mind equipped with an almost photographically persistent imagery, which may be contemplated and arranged at leisure. This is natural enough, when you have discovered it; but it is hardly what you would have argued *a priori*. I might cite a number of similar results: but the facts stand in isolation; in the main, imagination is virgin territory, and awaits as it invites the pioneer.

Of the more complex (8) *affective formations* we can say but little until we have an assured psychology of feeling. I

[1] "The Psychology of Thought." This Lecture is not here reprinted, as the author has recently published a more extended discussion of the subject.

remarked, earlier in this lecture, that Wundt has made significant use, in his theory of emotion, of the temporal aspect of mental processes; and I think that the future experimenter will do well to take the hint. But we cannot analyze emotion and sentiment, with any prospect of final success, until we are agreed upon the nature and number of the affective qualities.

It is a relief to turn from these topics, of imagination and emotion, to the problem (9) of *thought*. We have found various things that may with some truth be called 'characteristic' of the experimental psychology of the last ten years: the revival and extension of psychophysics, the focalizing of the affective problem, the emergence of a tangible psychology of attention, the establishment of the laws of memory. Nevertheless, if I were asked to name a single line of investigation that, more than any other, has characterized the decennium, I should not hesitate to select the experimental studies of the thought-processes, most of which we owe to the Würzburg laboratory. Not that these researches have been confined to Germany: on the contrary, Binet in France, Woodworth in the United States, Bovet in Switzerland, as well as Marbe and his successors at Würzburg, have all attacked the same problem; though it is true that the German work has been the most thorough and the most persistent. Here is a new departure in experimental psychology; concept and judgment and inference, the last refuge of the rational psychologist, have been ranged alongside of sensation and association, introspectively analyzed and made subject to the chronoscope. I shall endeavor, in my next Lecture, to give you some idea of methods and results, and to point out the most promising paths of future enquiry.

So I finish the principal part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. It remains, now, to say something of the extensions of the experimental method beyond the limits within which the present discussion has moved. What of individual psychology; of the psychology of the minor abnormalities—sleep, dreaming, hypnosis; of experimental æsthetics? What, last but not least, of comparative psychology?

I have time only for a word or two. Individual psychology, which was first systematized by Stern in 1900, is, in its modern form, one of the chief witnesses to the value of experiment. It furnishes the key to many, otherwise inexplicable differences of result, and it promises to allay many of the standing controversies of the text-books; there can be no

doubt that it will play a part of steadily increasing importance in the immediate future. The psychology of the abnormal seems, on the contrary, if I may venture an opinion, to have failed somewhat of its theoretical promise; recent interest has turned toward application. Vogt's method of 'direct psychological experiment in the state of hypnosis' has not helped us, as some had hoped it would, to an affective analysis; and although Martin and Ach have brought hypnosis into the laboratory, it does not appear probable that their example will be generally followed. Experimental æsthetics has shared in the recent horizontal expansion of psychology at large. A review of the work done upon colors and spatial forms, which appeared in the sixth volume of the *Année psychologique* (1900), mentions no more than five investigators; and there is an interval of more than twenty years between the first and the second, between Fechner and Witmer. But when Kuelpe came to prepare a similar review for the 1906 Congress of Experimental Psychology, he found a wide extension of subject-matter, a whole armory of methods, and a very considerable body of results. No doubt, the time is still distant, if indeed it is fated to arrive at all, when experimental æsthetics shall bear to a *System der Aesthetik* the relation that experimental now sustains to systematic psychology. In the meanwhile, it is satisfactory to know that, in this as in other directions, the American laboratories are doing their full share.

Again, however, there is something still more characteristic to follow: I mean, of course, the rapid growth of an experimental psychology of the lower organisms, a growth that is evidenced by the issue of books, the manifold publication of technical studies, and the invasion of our laboratories by various unwonted forms of animal life. The present status of comparative psychology has already been set forth by an authority far more competent than myself; and I desire now simply to give expression to my personal feelings, as the director of a laboratory primarily intended for the investigation of human consciousness. Personally, then, I welcome the animals, both for my own sake and for theirs. For my own, because I think that the comparative work exerts a wholesome influence upon the humanist. It is difficult to lay one's finger upon any definite point of indebtedness; but I am sure that many of my ideas have been quickened and clarified by acquaintance with the problems and methods of comparative research. And for the sake of the animals: because a comparative psychology can be built up only by men who have had their training in human psychology. Of that fact there can be no manner of doubt. The zoölogist may, and probably does, excel the psychologist in his mastery of biologi-

cal technique. But psychology also has its technique: psychology has its attitude, its point of view, its permissible and its impermissible questions, its methodological equipment, a technique that it takes time to acquire; and if you lack it, you may observe animal behavior with all the patience and all the accuracy conceivable, but you will not attain to a comparative psychology. Until such time, therefore, as experimental psychology forms a recognized part of every man's general biological training, training in physiology and zoölogy and the sciences of life at large,—until such time, the place for the animals who are to reveal the range and character of their mental endowment is the psychological laboratory.

I am almost at an end; and I have given you, I am afraid, little more than a string of names and a few general remarks; though, after all, one cannot do much in an hour. There is, now, one further point that I desire to discuss before I close. I have taken it for granted, throughout this lecture, that the primary aim of psychology is the analysis of mind. Yet a great deal has been written, of late years, against psychological analysis. Consciousness, we are told in effect, is a living continuum; but the analyst kills in order to make his dissection; and, after killing, he is unable to restore the life that he has taken, to show consciousness in its original integrity. This argument, if it were taken seriously, would apply to biology as well as to psychology, and would banish the muscle-nerve preparation and the microtome from the biological laboratory. But, indeed, it rests only upon misunderstanding,—a misunderstanding due in part to temperamental reaction, in part to the pressure of history and tradition. When the physiologist describes a tissue as 'composed' of muscle fibres or nerve cells, nobody takes him to mean that the fibres and cells existed first, in isolation, and that they were presently brought together, by some law of organic growth, to constitute the tissue. What grew was the tissue itself, which the physiologist now finds, in his *post mortem* examination, to consist of the cells or the fibres. Nevertheless, the analytical psychologist is supposed to generate his mind by allowing sensations to fuse and ideas to colligate, precisely as the physiologist might be supposed to generate a muscle by allowing the fibres to 'constitute.' In reality, to charge the analytical psychologist with deriving mind from the interconnection of the mental elements—and how often and how recklessly has not that charge been made! —is sheerly to misunderstand the purpose of analysis in the hands of those who use it.

The scientific legitimacy of the analytical attitude is, then, beyond dispute. Whether the results of analysis, in the sphere

of mind, are of 'value' is another question, and a question whose answer depends upon what one is disposed to consider valuable. What is psychology 'for?' If the object of the psychologist is to know mind, to understand mind, then it seems to me—in view of the overwhelming complexity of mind in the concrete—that his only course is to pull mind to pieces, and to scrutinize the fragments as minutely as possible and from all possible points of view. His results, in synthetic reconstruction, give him the same sort of intelligent grip upon mind that the analytical results of the physiologist give him upon the living body. To approach the study of mind without analysis would, in fact, be nothing less than ridiculous; and, in fact, no one does it. The most ardent advocate of mental integrity can follow up only one mental aspect or one mental function at a time.

I conclude that analysis is not only valuable, but indispensable to psychology; and I contend that many of the current arguments urged against psychological 'atomism' betray a woeful misunderstanding of scientific methods, and that much of the current depreciation of analytical results betrays a like misunderstanding of the aim of scientific psychology. But, in saying these things, I am only repeating what has been more aptly said by Ebbinghaus. And as I began this lecture by deploring the loss which Ebbinghaus' untimely death has entailed upon our science, so I cannot end it better than by reference to the admirable sanity which marks his treatment of the wider issues of psychology in the first volume of the *Grundzüge*.

AN EXPERIMENTAL STUDY OF IMAGINATION[1]

By CHEVES WEST PERKY

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INTRODUCTION

The word Imagination and its cognate forms are familiar both in everyday speech and in the technical language of psychology. In neither context, however, have they the established position enjoyed by the correlative term Memory. Under these circumstances, it seemed worth while to enquire into the psychological status of Imagination, to attempt an experimental control of certain of the experiences thus denominated, and by these means to work towards a definition and delimitation of the concept. The present study is no more than a first beginning, but we hope that its results are sufficient to justify the recourse to the experimental method.

Alongside of the experimental work, which will be described later, we undertook three preliminary enquiries: a somewhat

[1] From the Psychological Laboratory of Cornell University.

casual investigation of the meanings attached to the word Imagination by ordinary educated persons; a canvass of literary usage; and an analysis of the treatment of Imagination in current psychological text-books and monographs.

(1) *Ordinary Usage.* The verb 'to imagine' is used with many shades of meaning. We found, however, two fairly characteristic conversational uses: 'to imagine' is either to set an imaginary something before the mind's eye, or it is, more generally, to make an hypothesis, to suppose something of an unusual or difficult kind. Instances of a simple sort would be: 'Now you are to imagine a very long lane,' and 'Could you imagine anything worse?' The turn of mind which is commonly called imaginative is suggested by various proverbial expressions: counting chickens before they are hatched, crossing bridges before you come to them, building castles in Spain, etc. The imaginative person is also credited with a certain lightness or instability of temperament, though perhaps the only feature common to all mental constitutions classed popularly as imaginative is the negative one of a lack of thorough-going stupidity. Sometimes, as we have indicated for the verb, 'imaginative' has a visual reference; as a rule, however, this reference appears to be no more obvious to the speaker than it is in the case of the substantives 'insight' and 'foresight.'

(2) *Literary Usage.* One of the commonest uses of the word 'imagination,' in literature as in ordinary conversation (where, however, if our notes are to be trusted, it is far less common than the adjective and the verb), is to denote a tendency to distort facts, to embroider, to romance: *cf.* the phrase 'lively imagination.' Akin to this tendency is the ability to live in a subjectively colored world unlike the gray world of reality, a temperamental optimism or pessimism. With a very slightly different shade of meaning, imagination becomes the ability to see everything in a rosy light; or, again, the faculty of seeing things in the very high color that strong feeling paints. In some cases, the power of sympathy, of feeling with and for, is actually called imagination. Rich endowment on the creative as well as on the affective side of human nature produces the reformer, the prophet, the revolutionist, the visionary. Emphasis is often laid on this creative factor, the power of invention; imagination implies fluidity of associations, quickness of wit, the faculty of forming new combinations, the ability to see likeness (the one in the many) and to draw distinction (to see the many in the one). Finally, we note that the imaginative person is, with approximately equal frequency, described as the player, the dreamer *par excellence*, the purposeless idler, and as the possessor of an unusually high degree of concentration and voluntary control.[1]

(3) *Psychological Usage.* It is easy to overestimate the significance of a formal definition. At the same time, the framing of a definition marks a stage in the development of our knowledge of a subject,

[1] This paragraph is a condensed summary of the results gathered from a fairly wide study of poets, essayists, divines, and articles dealing specially with the Imagination. Of the latter we may mention, by way of illustration: H. Maxwell, Imagination, Blackwood's Edinburgh Magazine, cl, 1891, 576; R. C. Witt, The Imaginative Faculty, Westminister Review, cliv, 1900, 217; What is a Strong Imagination? The Spectator, lxii, 1889, 506; The Imagination and its Development, *ibid.*, lxv, 1890, 372; The Conservation of the Imagination, The Saturday Review, lxxxviii, 1900, 576; The Imagination, from Lectures by J. R. Lowell, The Century Magazine, xlvii (N. S. xxv), 1893-4, 716; J. Sully, Studies of Childhood: i. The Age of Imagination, Popular Science Monthly, xlv, 1894, 323. The greater part of the authors consulted wrote in English; but sporadic search among French and German writers did not reveal any striking differences in their use of the term.

while the definition itself has at least the value of a classificatory rubric. When James Mill writes that "an imagination is the name of a *train*; . . . nor is there any train of ideas, to which the term imagination may not be applied,"[1] we have our bearings within a self-consistent and comprehensive psychological system. It seems fair to argue, then, that the casualness and vagueness of the definitions of Imagination offered in current text-books and monographs imply a real uncertainty as to its psychological status. Imagination is "the representative function of mind,"[2] or "the faculty of reproducing copies of originals once felt"[3]; to imagine is "merely to think of an object,"[4] or "to construct with images."[5] More specifically, the creative imagination "consists in the property images have of gathering into new combinations through the effect of a spontaneity"[6]; "it is the power of developing an object by being absorbed in it; it is the power of self-development of an object when we live it or live in it."[7] Or it may be both of these things at once: imagination "is the art, spontaneous or reflective, of forming syntheses or mental combinations," while it is also "the power of giving or attributing existence to the representations, of transforming them into beliefs or actions, in a word, of objectifying them."[8] These and similar statements are plainly partial or tentative; they have their useful place in the discussions in which they occur; but they certainly do not possess any general validity. The clearest and most positive definitions that we have found are the following three. "Imagination in the wider sense," says Höffding, "is identical with the power of ideation; . . . in the narrower sense Imagination is the power of forming concrete ideas."[9] Here is the modern form of the representative faculty. Wundt modifies the definition, to make imagination a "form of apperceptive activity;" imagination is "a thinking in particular sense ideas," a "perceptual form of intellectual elaboration."[10] Finally, Baldwin introduces the affective factor; "imagination is the affective or felt apprehension of relations among images."[11]

Is there, then, any constitutive *differentia* of the imaginative consciousness, any marked character in which its contents differ from the contents of other typical consciousnesses? Most psychologists are silent upon this point. Külpe notes the "oscillation and migration, the shrinking and expanding, of the subjective images, when they are the result . . . of the free and fortuitous play of imagination;"[12] and Wundt emphasizes the "sensory vividness and picturableness of its ideas."[13] It would, however, be unsafe to generalize upon either basis.

Other principles of distinction have, indeed, been proposed. Thus

[1] Analysis of the Phenomena of the Human Mind, i, 1869, 239.
[2] J. M. Baldwin: Handbook of Psychology; Senses and Intellect, 1890, 213.
[3] W. James: Principles of Psychology, ii, 1890, 44.
[4] G. F. Stout: Analytic Psychology, ii, 1909, 260.
[5] L. Arréat: Mémoire et imagination, 1895, 156.
[6] T. Ribot: Essay on the Creative Imagination, tr. 1906, 330.
[7] W. Mitchell: Structure and Growth of the Mind, 1907, 353.
[8] L. Dugas: L'imagination, 1903, 3, 308.
[9] H. Höffding: Outlines of Psychology, 178; Psychologie in Umrissen, 1887, 223 f.: "das Vermögen zur *Neubildung* konkreter Vorstellungen."
[10] W. Wundt: Outlines of Psychology, tr. 1897, 262; tr. 1907, 298; Human and Animal Psychology, tr. 1896, 316; Physiologische Psychologie, iii, 1903, 577, 632; Mythus und Religion, i, 1905, 5 ff., 62 f.
[11] *Op. cit.*, 242.
[12] O. Külpe: Outlines of Psychology, tr. 1909, 185; Ueber die Objectivirung und Subjectivirung von Sinneseindrücken, Philosophische Studien, xix, 1902, 508 ff. Münsterberg speaks similarly of the *Unbeständigkeit der Vorstellung*, and of the *bunte Wechsel der Phantasiegestalten*, Grundzüge der Psychologie, i, 1900, 341. *Cf.* F. Paulhan, Psychologie de l'invention, 1901, 7.
[13] Physiologische Psychologie, iii, 1903, 632, 634.

Wundt writes that the distinguishing mark of imagination is "the mode of connection of ideas;" "in all cases connection takes place according to a definite plan."[1] We have "an essential mark of the apperceptive process in the positive characteristic . . . that it depends on a voluntary synthesis."[2] Jodl speaks, in the same sense, of a "voluntarily effected reproduction."[3] Külpe enters on more familiar ground in the statement that "imagination is characterized . . . by the realization that the given ideas present something new."[4] We find many passages to this effect: "the creative imagination requires something *new*—this is its peculiar and essential mark;"[5] "its two essential characteristics are originality and power;"[6] "the component ideas . . . are brought into new combinations;"[7] and so on. Bain thinks that the "grand peculiarity" of imagination, as distinct from thought, is "the presence of an *emotional* element in the combinations;"[8] a point of view which, as we have seen, is also represented by Baldwin. Ribot declares that "the imagination is subjective, personal, anthropocentric; . . . the understanding . . . is objective, impersonal, receives from outside."[9] This last is evidently a logical distinction, and tells us nothing of the mental processes involved. And the other peculiarities noted, voluntary synthesis, novelty of combination, and the presence of an emotion, are after all marks of difference within the intellectnal processes themselves, and cannot be considered as ultimate. If the presence of images might distinguish imagination from rational thought, as a voluntary synthesis distinguishes it from memory, Wundt would clearly make his point; but it is unquestionable that rational thought may itself proceed in terms of images.

Failing to obtain a satisfactory definition or *differentia* of Imagination, we may turn to the statistical method, and enquire how, in general, it is treated in the standard psychologies; what part-processes are said to make it up; under what general headings, or in what general context, it is placed. It is somewhat surprising that current psychologies rarely give a separate chapter to Imagination; in many of them, it does not even share a chapter heading; occasionally, the term does not appear in the index; not infrequently, its importance in the context is indicated merely by the use of a capital letter.[10] It is also, perhaps, somewhat surprising to find that its discussion may appear, almost as if by chance, under the rubrics Image or Representation, Thought, Feeling, and Will.[11]

As a type of *intellectual* consciousness, Imagination is usually treated under the general heading of Ideation, or more definitely under Representation, Reproduction, or Centrally Excited Sensation; occasionally under Association or Thought-connection.[12] It is almost always in close proximity to Memory. Emphasis is laid rather on its complexity of contents than on its uniqueness as an intellectual formation; we read, as in any discussion of the intellectual processes at large, of the part played by affective, volitional, and motor or kinæsthetic factors. A few instances must suffice. Ladd and Ribot stress the motor aspect:

[1] Physiologische Psychologie, iii, 1903, 631.
[2] Outlines of Psychology, tr. 1897, 261; tr. 1907, 299.
[3] Lehrbuch der Psychologie, 1896, 509; ii, 1903, 161 f.
[4] Outlines of Psychology, tr. 1909, 188.
[5] T. Ribot: Creative Imagination, tr. 1906, 5, 11.
[6] L. Dugas: L'imagination, 3.
[7] T. Ziehen: Introd. to Physiological Psychology, tr. 1895, 168. *Cf.* Mitchell, *op. cit.*, 350 f.; A. Bain: Senses and Intellect, 1868, 570.
[8] *Op. cit.*, 599.
[9] *Op. cit.*, 10.
[10] Nine such cases were actually counted.
[11] Actual count from 20 works, chosen at random from our list of authorities, gave the ratio 16:16:15:12.
[12] So T. Lipps: Grundtatsachen des Seelenlebens, 1883, ch. xx.

"especially must we insist upon the prominence of motor consciousness in the neural conditions of productive imagination;"[1] "my aim is to extend [the motor] formula, and to show that it explains, in large measure at least, the origin of the creative imagination."[2] Bain and the French authors insist on the emotional aspect; there is an "emotional condition of originality of mind in any department;"[3] we must recognize the existence, "beyond images, of another factor, instinctive or emotional in form."[4] Baldwin's paragraphs recur constantly to preference, intention, volition, regulation of will;[5] Ribot finds in voluntary activity the closest analogy to the creative imagination;[6] Jodl remarks that the conduct of association by the attention passes over, almost indistinguishably, into the activity of imagination.[7]

Imagination, it is generally agreed, must in some way be concerned with images. But opinion differs as to what constitutes an image; some writers appear to include in that category even the vaguest of abstractions. "These ideas of the imagination may be generalized," says Ziehen.[8] "When used as a sign, the image may be made so schematic as to contain nothing irrelevant; and the abstraction may be so great that the sign loses the character of a sample, and becomes an arbitrary sign like a word."[9] "The image, deprived little by little of its own characteristics, is nothing more than a shadow."[10] The point is even made that "imagination exercises itself as well in the domain of pure ideas or abstractions as in that of concrete realities."[11] It is plainly impossible to generalize Wundt's criterion of sensory vividness. Despite the terminology of imagery, the trend of opinion seems to be not only that "between the creative imagination and rational investigation there is a community of nature,"[12] but also that the line of separation between thought and imagination is shifting and uncertain.[13]

There is a marked difference between the treatment of imagination as an intellectual, and its treatment as an affective or volitional consciousness. Nearly as many writers offer the latter as offer the former treatment; but they do not offer it as a matter of course; they are plainly on the defensive. Hence they urge, with great insistence, that "the *emotional* factor yields in importance to no other;"[14] that "it is a character of imaginative creation . . . almost essential that it be preceded, prepared for and accompanied by affective phenomena;"[15] that "imaginative" are identical with "emotion-ruled combinations;"[16]

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[1] G. T. Ladd: Psychology Descriptive and Explanatory, 1894, 410.

[2] T. Ribot: Creative Imagination, 3, 99, 113, 248, 318 ff.

[3] A. Bain: Senses and Intellect, 593, 599 ff.; Mental and Moral Science, 586; note to James Mill's Analysis, i, 245 f.

[4] Ribot: *op. cit.*, 7 f., 12, 31, ff., 79 ff.; L. Arréat, Mémoire et imagination, 122, 167 f.; L. Dugas: L'imagination, 3 ff., 7; F. Paulhan: Psychologie de l'invention, 28, 43.

[5] Senses and Intellect, 223 ff.

[6] Creative Imagination, 9 ff.

[7] Lehrbuch der Psychologie, 1896, 508; ii, 1903, 161 f., 166. *Cf.* G. Spiller: The Mind of Man, 1902, 496 ff. (imagination as attention-determined); Ribot: Creative Imagination, 86 (identity of attention and imaginative synthesis); Dugas: *op. cit.*, 100; Paulhan: *op. cit.*, 67. Stout (Analytic Psychology, ii, ch. xi) treats of imagination together with belief.

[8] Introd. to Physiological Psychology, tr. 1895, 168.

[9] W. Mitchell: *op. cit.*, 364; *cf.* 371 and 185 f.

[10] Ribot: *op. cit.*, 18.

[11] Dugas: *op. cit.*, 1. As a rule, however, the authors halt on the hither side of this statement: *cf.* Dugas: 3 f. 105, 198, 334; Ribot: *op. cit.*, 18, 184 f., 193, 207 ff., 236, 251 ff. 92 f.; Arréat: *op. cit.*, ch. vi (*cf.* pp. 11, 103); Ladd: *op. cit.*, 415; Mitchell: passages, cited above; Lipps: *op. cit.*, 469.

[12] Ribot: *op. cit.*, 29.

[13] *Cf.* Ladd's discussion: *op. cit.*, 431 f.

[14] Ribot: *op. cit.*, 31.

[15] Paulhan: *op. cit.*, 28.

[16] Bain: *op. cit.*, 602.

that "thought is the representative or cognitive apprehension of relations among notions; imagination is the affective or felt apprehension of relations among images."[1] And emphasis on the *volitional* aspect, if again somewhat less frequent, is at least as insistent. "Memory images change under the influence of our feelings and volition to images of imagination:"[2] "the active imagination involves the exercise of will in some of its forms, whether it be the positive attempt to control the images of fancy or the merest supervision and direction of their play;"[3] not only is "imagination in the intellectual order the equivalent of will in the realm of movements,"[4] but the two are identical so far as the part (an important part) played by movements is concerned;[5] "intellectual creation is altogether analogous to voluntary activity;"[6] "the image is, under an elementary form, a complete volition."[7] ——

It must be understood that the summary here given is inadequate, not only to the subject of the psychology of imagination, but also to our own survey of that subject. We have made no mention, *e. g.*, of such matters as the distinction between passive and active imagination; and we have refrained from multiplying quotations. At the same time, we hope that the impression left upon the reader by the above paragraphs is essentially correct.

Our preliminary study has left us with mainly negative results. We have found the term Imagination used very variously both in ordinary speech and in the psychologies, we have found scarce and inadequate definitions, no distinctive mark or marks of a reliable kind, and a pretty even distribution of emphasis among the four psychological rubrics of Idea, Thought, Feeling and Will. The very character of these rubrics, which are an inheritance from the faculty psychology, shows that no really analytical work has been attempted; psychologists have relied, for an account of imagination, upon their own self-observation and reflection, upon tradition, upon the requirements of a system. This is not to say that their accounts are wrong; on the contrary, they probably contain a good proportion of truth; but it means that there is no way of separating fact from fiction; we must accept an image-theory, or an affective theory, or a motor theory, or a volitional theory, in large measure by faith, or we must try to keep in the safe path by eclecticism and compromise.[8]

These are precisely the circumstances under which, if we may judge by historical analogy, assistance may be expected

[1] Baldwin: *op. cit.*, 242.
[2] Wundt: Outlines of Psychology, tr. 1897, 261; tr. 1907, 299.
[3] Baldwin: *op. cit.*, 226.
[4] Ribot: *op. cit.*, 9.
[5] *Ibid*, 11.
[6] Paulhan: *op. cit.*, 6.
[7] Dugas: *op. cit.*, 100.
[8] No one, for instance, can read without psychological profit such a work as E. Lucka's *Die Phantasie, eine psychologische Untersuchung* (1908); but no one, we suppose, will doubt that the writer's analyses and classifications are provisional and temperamental, rather than objective and final.

from the experimental method. But experiment must begin with the relatively simple, and not with the complex. The following sections are offered as a contribution towards that *Elementaranalyse der Phantasie*, which Wundt has prescribed as the foundation of a psychology of Imagination.[1]

§I. A Comparison of Perception with the Image of Imagination

The object of our first experiments was to build up a perceptual consciousness under conditions which should seem to the observer to be those of the formation of an imaginative consciousness. A visual stimulus was presented, gradually and with increasing definiteness, while the observer was asked to imagine the object whose color and form were thus given in perception. Hence by 'image of imagination' we here mean, not the elementary image-process that is co-ordinate with sensation, but such an image arises in a mind of the visual type at the command, *e. g.*, 'Shut your eyes and think of an orange.'

The Cornell Laboratory possesses a dark room, 5 x 6.5 m., which is set longwise to the middle of a light, gray-tinted room of considerably larger size. The dividing wall contains, at its centre, a window of 1 x 1.5 m., which is filled with a sheet of ground glass, and which may be closed on the light-room side by two swinging shutters that can be made to stand at any angle. For the purposes of the present experiment, a black cardboard screen, with a central opening of 36 x 36 cm., was placed in the dark room immediately behind the ground glass. Facing the glass, in the dark room, was a projection lantern, whose arc-lamp was replaced as occasion required by various types and powers of incandescent lamp. The light room is profusely supplied with incandescent lamps; and after some preliminary trials with daylight, we decided to use this artificial light throughout the experiments.

Our first problem was to determine the color-limen for the various stimuli that we intended to employ, and we succeeded in making this determination for a certain red, orange, deep yellow, light yellow, green and blue. The observer sat, in the light room, at a distance of about 8 m. from the ground-glass window. It was the task of the experimenter to vary the luminosity of the lamp in the lantern, the screens of ground and colored glass, colored and colorless gelatine, and white tissue paper, necessary to reduce the light from the lantern, the distance of the lantern from the window and from these various media, and (on the other side of the ground-

[1] Mythus und Religion, i, 1905, 17 ff.

glass window) the illumination of the observation room, in such a way that the open square should appear just noticeably colored, without there being any such glow or shine upon the glass as could suggest the presence of a source of light behind it. After a great deal of empirical testing, this end was attained, with a satisfactoriness and a precision that we ourselves—discouraged by repeated failures—had at last not dared to expect. Serial determinations of the limen of hue were obtained from Professor Titchener and the writer; a number of less systematic observations were made by Professor Bentley. The apparatus, we may repeat, was clumsy and empirical; but it worked (apart from errors of manipulation, to which we later refer) with admirable sureness and delicacy. We were especially on the watch for changes of tint; but we were able to eliminate them, positively for ourselves, and at least so far that they escaped notice for all of our observers.

The limen was thus determined as a diffused flush of color over the open square. The next step was to shape this flush into the representation of some object of perception. We prepared a set of black cloth-covered screens, in which were cut the forms of certain familiar objects; the forms were thus represented by holes in the screens. The edges of the forms were softened in outline by layers of fine black gauze, which projected successively farther and farther into the holes. The screens themselves were hung upon a rigid cross-line, along which they could be silently shifted in or out of their place behind the open square. A solid screen was used, to fill the square, while the stimulus-screens were in motion. When this solid screen was removed, the colored light shone through the reducing and diffusing media and the stimulus-screen, and the faintly colored and hazily outlined form lay upon or rather within the background of neutral gray.

The stimuli were presented in a definite order: a tomato (red), a book (blue), a banana (deep yellow), an orange (orange), a leaf (green), a lemon (light yellow). The adoption of a fixed procedure was necessary, since the apparatus required the services of three experimenters, and confusion might easily arise. One experimenter had charge of the lantern-lights, colored and ground glasses, colored and colorless gelatines, and tissues. A second ran the stimulus-screens into place, gave them a very slight, slow motion during their exposure (in imitation of the oscillations of a 'subjective' image), and replaced them on signal by the solid screen. A third sat in the light room, with the observer, to give instructions, take down introspective reports, and signal to the experimenters in the dark room for the appearance or removal of a particular stimulus. The electrical signal-

apparatus was arranged on the floor, under the desk at which this third experimenter sat; as the wires were concealed, and the experimenter's hands were free, the connection with the dark room was, unless mistakes of manipulation occurred, not suspected by any observer.

The experimenter who was in charge of the lantern had an exact table of the changes required to raise the color-stimulus from a definitely subliminal to a moderately supraliminal value. This table was based upon the serial results obtained from the observers mentioned above. When the signal for a particular stimulus was given, the color-stimulus was exposed, step by step, as the table prescribed, and in a tempo that had been standardized by practice. That the observer, in reporting an image, really perceived the stimulus, at any rate in the great majority of cases, seems to us to be proved by the fact that in only one single instance, throughout the entire series of successful experiments, did an observer report an image before the stimulus was (1) supraliminal for the co-operating experimenter and (2) of such objective intensity that its perceptibility might be expected from the results of the preliminary control experiments. It may, of course, be objected that this proof is not demonstrative: the experimenter may have been suggestible, and the position of the limen may vary considerably from observer to observer. At the end of the enquiry we accordingly took control observations from several of our graduate observers; and we found (3) that, when the arrangement of the experiment was explained to them, so that they were in the position of the co-operating experimenter in the actual experiments, they invariably recognized the appearance of the stimulus at or before the point at which they had previously reported an image of imagination.

Care was needed that no sound should come from the dark room; that the forms should oscillate or flicker into view very gradually; and that no shimmer of light should show between the edges of the screens as the stimuli passed in and out. The first of the sources of error was easily avoided; mistakes in manipulation occasionally occurred, however, and were, naturally, fatal to the success of the experiment with the observer in question. They will be noted in detail in what follows. We discovered no other source of error in the experiments.

A white fixation-mark was placed on the ground glass window at the centre of the open square. The work was done, as we have intimated, in the evening and by artificial light; all preparations were carefully made beforehand, so that the observer supposed that he and the (third) experimenter were the only persons present in the laboratory at the time of ob-

servation. Instruction was made as simple as possible; the observer was merely told to fixate the white point, and to hold this fixation while he 'imagined' a colored object,—"for instance, a tomato." He was then to describe his 'image,' if any image took shape. As soon as the description was begun, the attention of the observer was distracted from the window by some indifferent question ('What was that, once more'? or 'Quite clear, did you say'? or something of the kind), and at the same moment the experimenter signalled to the dark room for the turning out of the lantern-lamp or the swinging into place of the solid black screen.

No blank experiments were introduced, as we feared that their introduction would reveal the actually perceptual character of the induced 'image.'

Experiment I. Partly in order to rehearse the technique of the experiment, partly in order to see what would happen with wholly unsophisticated observers, our first observations were taken from three children; two girls of 13 and 14, and a boy of 10. The elder girl took the perceptions, as a matter of course, for images of imagination. The younger girl was excited and pleased by the 'images,' but had no suspicion of their perceptual character; she was astonished and chagrined when informed, the next day, of the arrangement and object of the experiment. The boy caught a flash of light (due to faulty manipulation in the dark room) early in the course of the experiment, and jumped to the conclusion that he was seeing 'shadows' cast somehow upon the screen.

Experiment II. Full sets of observations were obtained from a group of 27 observers, of whom 19 were sophomores in the university, and 8 were graduate students engaged in advanced work in the laboratory. Only 3 of these 8, however, were familiar with their furniture of images, and had worked experimentally on the topic of imagery.

Three of the undergraduate observers were ruled out, early in the experiment, by an error of technique, which was at once pounced upon and reported. The remaining 24, men and women alike, invariably mistook the perceptual for the imaginative consciousness. At the end of the series, after all the introspections had been recorded, the observer was asked whether he was 'quite sure that he had imagined all these things.' The question almost always aroused surprise, and at times indignation. Yet when asked, further, if he had ever had such images before, he would usually reply that 'he could not remember that he had; but then, he had never tried.'

It is unnecessary to quote the introspections in detail. The following remarks are taken each from the report of a different observer.

"It seems strange; because you see so many colors, and know that they are in your mind; and yet they look like shadows." "I can spread it [the color] over if I want to." "It is a pure memory, with a little effort I could move it to the wall." "It is just like seeing things in the dark; I had it in my mind." "It is just as imagination makes it." "I can get blue better, because I have been working with a blue square lately." "I can get it steadily so long as I keep my mind absolutely on it." "I can get the shading on it as I think of it; at first I think it flat, as if painted." "I can see the veining of the leaf and all." "The banana is up on end; I must have been thinking of it growing." "It is more distinct than I usually do [than the images I usually have]; but I have never tried much." "I got it; that was grand." "I am imagining it all; it's all imagination." "Feels as if I was making them up in my mind." "I get thinking of it, and it turns up."

Several times an observer remarked, towards the end of the series, that his images were something like after-images, and that he felt he could move them by moving his eyes; here, no doubt, the slight oscillation and fluctuation of the stimulus were in play. One graduate observer apologized for her 'poor imagination,' and said she could get forms but not colors; as a matter of fact, she failed to see the color of the stimulus even when it was increased very considerably beyond the ordinary supraliminal point; the forms she regarded as imaginative. Another graduate observer, who had had long experience in the laboratory and had worked to some extent with imagery, showed, both by the time of appearance of the image and by its characteristics (shape, position, size), that he was incorporating the perception in it, while he nevertheless supplied a context of pure imagery: the tomato was seen painted on a can, the book was a particular book whose title could be read, the lemon was lying on a table, the leaf was a pressed leaf with red markings on it. All the observers noted that the banana was on end, and not as they had been supposing they thought of it; yet the circumstance aroused no suspicion. Some saw an elm leaf when they had been trying for a maple leaf. The observers not infrequently volunteered the statement that they could continue to hold the image after closing the eyes.

There is, then, no ambiguity about the results. The experiment is, however, open to the objection that the observations were made, for the most part, by unpractised observers.

Experiment III. To meet this objection, we repeated the experiment, after a year's interval, with five graduate students: *K*, *S*, *T* (men), and *C* and *V* (women). All had had practice, and *T* and *V* unusually extended practice, in the observation of images.

S was for some time confused: at first he thought the

figures imaginary; then he speculated whether they might not be after-images of some sort, or akin to after-images. Finally, after the appearance of the fourth form, he remarked: "It seems like a perception, though the attention is more active than in perception; yet I feel sure that it is there, and that I did not make it; it is more permanent and distinct than an image." The permanence and distinctness were, unluckily, due to faulty technique; the signal to put out the light failed to carry its message, and the stimulus was left showing at rest.

T thought that the 'image' was rather more like an after-image than the images he was accustomed to get in daylight; but he was emphatic that it was not a perception. *K*, too, insisted that the figures were imaginative.

C took the perception for an image of imagination until a mistake of technique (with the fourth form) revealed its real nature. *V* added many imaginary details, such as 'printing' on the book, but was sure that the figures were "all imagination, of course"; "quite like those I get in the daytime—perhaps more normal"; "more strain than in a perception" (*cf.* *S*'s active attention); "could feel it formed in my mind—came right out of me"; "if I hadn't known I was imagining, should have thought it real."

The results thus confirm those of the previous experiments. And the net outcome of the work is, we think, positive and important enough to justify the time and labor spent upon its preliminaries. We find, in brief, that a visual perception of distinctly supraliminal value may, and under our conditions does, pass—even with specially trained observers—for an image of imagination. We are at a disadvantage in not being able to express our stimuli in quantitative terms. This much may, however, be said: at the conclusion of the experiments, a demonstration of the colored forms was made to a number of students in the psychological department, and to some psychologically competent visitors. Every one of these observers showed great surprise; most of them 'would, of course, take our word' for the facts, but could hardly credit them; a few remained entirely incredulous—'we asked them to believe too much.' These attitudes are sufficient warrant for the normally supraliminal character of the stimuli employed.

Historical Note. Münsterberg concludes from his experiments on the mis-reading of words "dass die im normalen Zustand reproduzierten Empfindungen unter günstigen Bedingungen von sinnlichen Eindrücken nicht unterschieden werden können" (Beiträge zur experimentellen Psychologie, iv, 1892, 22). Külpe remarks that "the limits of possible variation in these experiments are plainly restricted; and the experiments themselves cannot be considered as wholly free from objection . . . The observer's statement that he clearly saw the whole

word is hardly a guarantee that errors [of the kinds specified in the criticism] were eliminated" (Outlines of Psychology, tr. 1909, 183). Pillsbury's results, gained from similar experiments, "seem to show that the centrally excited sensation, no matter how certain of its existence we may be, possesses certain characteristic differences that distinguish it from the peripherally excited." "Letters wrongly completed were said, *e. g.*, to be of a different color from those which were actually present upon the screen; were less definite in outline; and were less stable, *i. e.*, seemed to be in motion over the word." Pillsbury adds that the method is not suited to the problem; the observer is interested in the word as a whole, not in the details of the letters (this *Journal*, viii, 1897, 367 ff.).

Külpe had made experiments on the question before 1892 (Philosophische Studien, vii, 399), and gave a brief account of them in 1893 (see Outlines, 182 ff.). They were fully reported in 1902 (Philosophische Studien, xix, 508 ff.). The observer, placed under experimental conditions, was asked to say whether he saw or felt anything, and if so what it was like, and whether he thought that it was objective or subjective: the stimuli actually given were a faint illumination of the walls of a dark room, and a pressure upon a pressure spot, with variations of interval, duration, intensity, etc. Külpe concludes that the criteria of subjectivity and objectivity are neither immanent nor universal; so that the predicates objective (referable to an external stimulus) and subjective (attributable to the condition of the observer) are always secondary and empirical determinations. We may point out that our own experiments were, in a way, the converse of Külpe's; his instruction was to report any sight or touch, and to say whether it was a matter of perception or of imagination; we asked definitely for an imagination, and sought to substitute for this, or to incorporate in it, an otherwise supraliminal perception. By thus demanding an image, we gained a further advantage; for Külpe's method does not guarantee the arousal of 'centrally excited sensations.' Külpe himself declares, àpropos of his visual experiments: "inwiefern auch central erregte Empfindungen dabei eine Rolle gespielt haben, wage ich jetzt nicht zu entscheiden . . . Dass an der Vermengung mit objectiven Erscheinungen auch central erregte Empfindungen mitgewirkt haben, ist wahrscheinlich" (P. S., xix, 528, 533). Our experiments evidently called up images in all those cases in which the perception was supplemented (lemon *on table*, etc.); while in the other cases, whether or not an image was called up, there was a presentation which simulated and was identified with an image—and this under the most careful scrutiny and with sustained attention. For the rest, it is worth while to note that the differences between perception and image of imagination found by Pillsbury are all of the kind described by Külpe as secondary or empirical.

Washburn, in her study of subjective colors and the after-image (Mind, N. S., viii, 1899, 32), remarks that "perception and idea differ ultimately only in the manner of their production. . . . Where the intensity of the peripheral process is reduced to a minimum, the resulting conscious state is seen to be practically identical in character with that produced by central excitation." And there are many other experimental indications of the introspective identity of sensation and image: the nature of the visual images in Galton's 'highest class;' the possibility of securing an after-image from a 'mental image' (Downey); the probability of the intervention of images in certain normal illusions (Seashore and others); the facts of the memory after-image, of synæsthesia, of hallucination. It is not our intention to review these topics, or to enter upon the question of the cerebral seat of sensation

and image: all this would carry us too far afield, and the time is not ripe for full discussion of the image of imagination upon an experimental basis. We wish only to remind the reader that our experiments have a positive, if fragmentary, experimental background.

Much has been made of Lotze's statement that "the idea of the brightest radiance does not shine, that of the intensest noise does not sound, that of the greatest torture produces no pain" (Outlines of Psychology, tr. 1886, 28; Medicinische Psychologie, 1852, 477 ff.; Microcosmus, i, 203 f.). Titchener has recently remarked that a view of this sort, in so far as it depends on an intensive comparison of sensation and elementary image (a thing impossible, for most persons, in the case of pain), is probably due to the intercurrence of the stimulus error (Experimental Psychology of the Thought-processes, 1909, 267); and experiments now (March, 1910) in progress in the Cornell laboratory serve definitely to confirm that hypothesis. Ziehen's dictum that "the ideas of the slightest rustling and of the loudest thunder exhibit no difference in intensity whatever" (Introd. to Physiol. Psychology, 1895, 154; Leitfaden, 1906, 136) is flatly untrue if the rustling and the thunder are reproduced in kind; if, that is, images of auditory imagination replace auditory perceptions; for auditory images, as will be shown in a later communication from the laboratory, possess the attribute of intensity. The very fact that statements such as these can be made witnesses to the extreme need of experimental observation.

In general, of course, the experimentalists incline to the view that there is no intrinsic difference between sensation and image. It is not our intention, again, to review this topic; the review would be premature. We refer only to A. Meinong's classical discussion in the Vierteljahrsschrift für wissenschaftliche Philosophie, 1888-9, Nos. 11, 12, 13; and to Münsterberg, Die Willenshandlung, 1888, 138 f.; Grundzüge der Psychologie, 1900, 311; Külpe, Outlines, 85, 183 (it is to Külpe, so far as we are aware, that psychology owes the terms 'peripherally excited' and 'centrally excited sensations'); Stumpf, Tonpsychologie, i, 1883, 260, 375 f.; Wundt, Human and Animal Psychology, tr. 1896, 14; Outlines of Psychology, tr. 1907, 31, 39; Physiological Psychology, i, tr. 1904, 13 f.; Pillsbury, Attention, 1908, 95; Titchener, Elementary Psychology of Feeling and Attention, 1908, 337; Text-book of Psychology, i, 1909, 198 f.

§ II. Kinæsthetic Elements in Images of Imagination and Images of Memory

Our next concern was to bring the image of imagination into relation, for purposes of introspective comparison, with images of memory. As a stimulus to the image of imagination we could think of nothing more promising than the spoken word. There is, truly, no guarantee that the spoken word will arouse these images; but it seemed practically certain that, if a fairly large number of experiments were made, the two types of image would be found among the results. The observers were accordingly seated in a dark room, and instructed to give themselves up to the visual imagery evoked by the word; they were not in any way to control or regulate their imagery, but were to give it rein, passively and as association determined. No classification *a priori* was attempted.

It soon appeared that a good proportion of the images thus

aroused were of two sharply different kinds. There were, on the one hand, images of recognized and particular things, figuring in a particular spatial context, on a particular occasion, and with definite personal reference; and there were, on the other hand, images with no determination of context, occasion, or personal reference,—images of things recognized, to be sure, but not recognized as this or that particular and individual object. The former were evidently 'images of memory;' the latter, both by positive and by negative character, were 'images of imagination.' In other words, we have in our results a rough and ready criterion of the two types of image: memory being distinguished by particularity and personal reference, and imagination contrasting with it by lack of particularity (in the sense of a particular sample, placed and dated) and absence of personal reference. There were, naturally, a fairly large number of intermediate forms (images with personal and place references, but unfixed in time; images with personal reference, but neither temporal nor spatial context; images with context but no personal reference). The classification of these under the one or the other of the two main rubrics would have been possible, from the records, although it would have left a margin of uncertainty, aside from that due to the possibility of an incomplete introspective account. Fortunately, however, we had no need to attempt it, as the clear-cut cases were sufficiently numerous for our purpose.

One of the first things noticed, as we sought to analyze the images, was the presence of certain kinæsthetic factors in the image of memory which were not traceable in that of imagination. Sensations of eye-movement were by far the most obvious; memory appeared to imply roving movement of the eyes, while imagination implied steady fixation. The indications were that other and more general kinæsthetic differences obtained, but this, of eye-movement and fixation, was the most noticeable. In view of the important part played in theory by the kinæsthetic factor, we decided to put this discovery to the test of experiment.[1]

The observer sat in a dark room, his head supported in a

[1] We naturally think of the seer and dreamer as rigid, in a fixed stare; and we know that the effort to remember sends our eyes wandering over walls and ceiling, as if we hoped somewhere to find a cue to memory. *Cf.* F. Meakin: Mutual Inhibition of Memory Images, Harvard Psychological Studies, i, 1903, 244; C. S. Moore: Control of the Memory Image, *ibid.*, 296; J. W. Slaughter: Behavior of Mental Images, this *Journal*, xiii, 1902, 548; F. Kuhlmann: Analysis of the Memory Consciousness, Psychological Review, xiii, 1906, 338 f.; E. Murray: Peripheral and Central Factors in Memory Images, this *Journal*, xvii, 1906, 241; Külpe: Outlines, 1909, 187; etc.

headrest, and his left eye screened. The right eye fixated a luminous (phosphorescent) spot 1.5 m. distant. Four other and similar luminous spots, at the same distance from the eye, were placed within and near the upper, lower, right and left margins of the blind spot of the fixating eye. A sixth luminous spot was placed to the right, just beyond the limit of the field of vision. So long as fixation was continued, or the fixating eye moved only within certain narrow boundaries, the outlying lights remained invisible; so soon as the movement of the eye attained a certain extent, some or all of the outlying lights flashed into the field.

The observer was required, first, to gain such control of fixation that the five outlying spots remained permanently invisible when the head was in position; there was some difficulty with all observers, but it was overcome by practice. In the experiments themselves, a signal was given, and then the word was called out to the observer, who signalled (by a minimal hand-movement, which did not at all disturb fixation) the appearance of an image. Careful and detailed introspections were taken of the course and character of the image, and a note was made of the appearance or non-appearance of outlying lights in the field while the image was in course. The time elapsing between the giving of the stimulus word and the arousal of an image was recorded by the experimenter; but no use will be made of this determination in the present paper. The observers were left altogether in ignorance of the object of the work; they soon discovered that the movements of the eye were in question, but no one of them, so far as we are aware, realized that we were in search of a distinction between image of memory and image of imagination. These terms were carefully avoided; the experimenter spoke only of 'image.'

Obvious sources of error were: the limit of function of the apparatus, the possibility that the observer should forget the appearance of outlying lights in his anxiety to do justice to the image, and the occurrence of intermediate and dubious forms of image. In so far as the apparatus allowed of slight movements, without sight of the lights, its limitation would serve to increase the number of observations of 'imagination with no movement,' and to reduce that of observations of 'memory with movement.' Yet the movement that would bring in the lights was very slight, and the movements involved in the memory image were gross; so that we do not ourselves attach weight to this source of error. The second we must accept; though the training and attention of the observers may be supposed to reduce it to a minimum. It appeared, without suggestion from the experimenter, in the introspections of the sentence-experiments (Experiment VI), and is there allowed

for. The third we avoided by discarding all equivocal results in our final calculations. Lest, however, this procedure should seem arbitrary and perhaps unfair, we sought in every instance to make a distribution of the doubtful cases, as impartially as possible, to the two main categories (an example is given in the auditory experiments: Experiment VIII); and in every instance the outcome of the combination told for and not against our main thesis.

Experiment IV. The observers, Messrs. Geissler, Tsanoff, and Williams, and Misses Clarke, Day, and Rand, were all graduate students; only *D* and *T* had had practice in the observation of images. After ruling out all equivocal images, 5 cases of images of printed words, and 16 cases in which mistakes of technique occurred, we had 426 experiments of which 212 gave images of memory (in the sense explained above) and 214 gave images of imagination. These results were somewhat irregularly distributed among the observers, but no observer reported less than 20 images of each kind.

Of the 212 memory images, 191 (or 90%) involved eye-movement, to the extent that outlying lights came into the field of vision; of the 214 images of imagination, 146 (or 68%) showed no evidence of movement. Of the 68 images of imagination for which movement was recorded, 12 were images of animals conceived as running swiftly across the field of vision, and 17 were images of greater extension than could be compassed by the resting eye,—images of a stretch of landscape, or of the side of a near and large building. If we rule these out as equivocal, we have only 21 anomalous cases of memory (no movement) and 39 anomalous cases of imagination (movement recorded).

Experiment V. In a second experiment, the experimenter selected the stimulus words with a view to the avoidance of very extended images, and of images of moving animals. Minor improvements in technique (size and permanence of the phosphorescent spots) were also effected. In spite of preliminary practice, some of the observers had reported a certain difficulty in the presence of the fixation spot; the peripheral stimulus seemed to interfere with the formation of the image. At the conclusion of the first experiment, however, this difficulty had been overcome; so that, on the whole, better results might now be expected. The observers were as before.

Of 227 memory images, 218 (or 96%) involved eye-movement; of 165 images of imagination, 122 (or 74%) showed no evidence of movement. But, in spite of precautions, 30 of the 43 refractory cases of imagination were cases in which the observer imaged an extended scene. Of the 122 cases without movement, 6 were also of extended scenes; but here no estimate could be made (as was ordinarily the case) of the distance of

the scene from the observer. In 10 of the remaining 13 cases of imagination with movement, the observer reported a fatigue of fixation before the appearance of the image. There were no other discoverable sources of error in the experiment.

Experiment VI. It did not seem probable that the weight of evidence could be increased by further experiments of this kind. There was, however, a possibility that the use of the single word as stimulus induced an artificial or abnormal steadiness of fixation, which would not exist if a series of images of imagination were passing through consciousness. We accordingly turned to serial stimuli; we employed sentences, bits of nursery rhyme, passages from Shelley's Cloud, Lanier's Song of the Chattahoochee, Kipling's Chant-Pagan. These were read slowly and distinctly by the experimenter; the observer signalled the appearance of an image, and the corresponding word was noted by the experimenter; introspections were taken at the end of the reading. The observers were *R*, *T* and *W*.

Of 94 memories, 68 (or 72%) involved eye-movement; of 269 imaginations, 243 (or 90%) showed no movement. In 10 cases (thrown out) the observers could not remember whether or not the lights had flashed into the field of vision. In the other cases, they felt fairly certain of their report; though the affective state aroused by the stimuli was at times so strong, and the interest in the images so intense, that peripheral stimuli might, perhaps, pass unnoticed. We must leave this possibility undetermined.

As might have been foretold from the imaginative character of the stimuli, there is a great preponderance of images of imagination. In many cases, an existing image or fusion of existing images served to form a context for a newly entering image, which still remained positively an image of imagination; the ready-made context did not make it over into an image of memory. Sometimes there was, in these cases, a sort of personal reference, but it was radically different from the reference of memory; the observer would 'see himself there' rather than feel himself there; he seemed detached from himself, filling "a blank space which represented himself;" "there was somebody in the boat which [*sic*] I supposed was myself." By this time, indeed, the observers had frequently remarked on a conscious difference in types of images, noting that "sometimes the context seems the important thing, and sometimes the image itself," and that a 'particular' image seemed to be of a different mental order from an unreferred image. It was time, then, to transcend the preliminary and non-psychological criteria that we had so far relied upon, and to seek to ascertain the introspective basis of this difference.

Experiment VII. Comparatively few observations were taken in this Experiment, as the introspective reports were made as full as possible. The observers were *D*, *T* and *W*. The word method was employed.

Of 39 memories, 35 (or 90%) involved eye-movement; of 61 imaginations, 53 (87%) showed no movement. Other determinations were as follows:

100% of imaginations reported that fixation was apparently necessary before images could be secured;

75% reported lively affective processes;

92% reported great vividness of imagery;

93% reported a 'feeling of personal detachment;'

41% reported a mood of strangeness or novelty.

Introspective comparison of the two types of image brought out the fact, for all observers, that "the difficulty in holding the fixation is greater with the particular than with the generic images ['generic' is a term employed spontaneously by the observer quoted]. The latter seem to require fixation anyway, it seems to build them up before your eyes as you gaze steadily, while for the others you seem to want to look around, or to be free in some sense, and the fixation-point seems to hamper you."

Experiment VIII. It was still a question, however, whether the kinæsthetic factor present in the process of memory were peculiar to visual imagery; and whether it were a responsive movement, or a preparatory movement of adjustment of the sense-organ involved. The latter question might also, in principle, be asked of fixation. We accordingly arranged an Experiment for testing the laryngeal movements that are associated with inner audition. The observer assumed a semi-recumbent position on a couch, the inclined portion of which supported the upper part of the body and the head. We hoped through this position to avoid head and body movements, as well as general muscular strains and fatigue. The Verdin laryngograph, rigidly supported, was applied to the larynx, and so adjusted that any movement which fell within its range of sensitivity could be recorded on a kymograph. Record was made, on the same surface, of the appearance of auditory images in the mind of the observer. The arm and hand signalling these appearances were comfortably relaxed, and preliminary tests were taken to make sure that the signal movements did not change the laryngographic tracing.

The observers were *D*, *W* and Mr. Nakashima; all graduate students. Three other persons who offered their services were rejected, since they were unable to obtain auditory images. Another had good images, but gave no record of laryngeal movement under any conditions; he had been a singer as a

boy, and had overstrained his throat,—a fact which may account for this absence of appreciable movement. The preliminary work with these rejected observers was useful as affording practice in the manipulation of the laryngograph. The word method was employed.

Of 155 memories, 84% (minimum 80%, maximum 90%)[1] involved movement; of 214 imaginations, 91% (minimum 90%; maximum 96%) showed no movement. There were 56 doubtful or equivocal cases. We have sought, on the basis of the very full introspections, to distribute these as fairly as possible to the two main categories; with the result that memories with movement rise to 86%, and imaginations without movement to 94%.

The averages here, however, are a little misleading. For *D* and *W*, the most practised observers, at no time recorded movement with images of imagination; and *D* gave but one case of memory without movement, while *W* reported only 6 such cases. *D*'s record was especially clear, the movement showing conspicuously in the tracing with the memories, and being as conspicuously absent with the imaginations.

This Experiment was repeated, with like result, after an interval; there were, again, three observers. We do not think it necessary to report the details.

Experiment IX. One of our observers, Miss de Vries, had mentioned that strong olfactory images were of common occurrence in her experience. Experiments were therefore made, again by the word method, in this department of imagery. The observer lay on the couch, in the position described above, with eyes closed, and signalled the appearance of an image. The experimenter carefully watched the observer's face for signs of movement. Introspections were taken after the image had run its course.

Of 56 memories, 96% involved movement of the nostrils, and 86% very definite movements, a sniffing in and out of the nostrils and a jerking of the head. Of 57 imaginations, 46 or 80% gave no perceptible movement. Of the remaining 11, 9 involved only a very slight twitching of the nostrils, just observable by the experimenter; and 5 of the corresponding images had a local visual setting, although this was not definitely recognized.

We lay no great weight upon these results, though they are evidently in accord with those obtained with vision and audition.

[1]The record was delicate, and not always easy to read or interpret. Hence we give the possible minimum and maximum, as well as what seems to us and to our assistant readers the fairest percentage.

No experiments were made upon the imagery of touch.

If we summarize the results of the foregoing Experiments, we have the following:

No. of Obs. Memory	Imagination	Imagery	Percentage of Memory and Movement	Percentage of Imagination and No Movement
572	709	Visual	89.5	79.5
155	214	Auditory	84	91
56	57	Olfactory	96	80

As they stand, the figures are significant enough. But it must be remembered that we have, throughout, given the observation the benefit of the doubt; we have made no distinction between stages of practice, we have allowed full weight to discrepant observations which nevertheless could readily be accounted for (running animals, etc.). In other words, our figures are as low as they can be made, and might easily and without undue pressure of interpretation have been made higher.

§III. Affective Factors in Memory and Imagination

It would be hasty to conclude, from the foregoing Experiments, that the imaginative consciousness is constituted as such by a typical distribution and proportion of kinæsthetic elements, and by this alone. Emphasis has been laid, in many quarters, upon the affective component in imagination, and it is necessary that the affective theory be brought to the test of experiment.

We thought, at first, of having recourse to the method of expression. But we gave up this idea: partly on account of the perplexity in which the method is itself involved, partly because we did not see how, at its best, it could help towards a solution of our problem. The questions at issue are whether there is a qualitatively characteristic mood which informs consciousness in memory and in imagination, and whether—granted that the moods exist—the imaginative consciousness is more strongly, more markedly affective than the memorial. We knew from Experiment VII that the word method could bring out reports of mood, and we accordingly adopted it, though in slightly modified form, in the present case.

Experiment X. We selected two observers who had vivid visual imagery: Miss de Vries, of whom we have already spoken, and Dr. Pyle, an assistant in the laboratory. Both observers were familiar with the standard investigations of mental imagery; and both had had practice in this field of observation. The new experiments were made in diffuse daylight: the observers sat at about 1.5 m. from a buff-colored blank wall; their eyes were open, but fixation was not prescribed. The general instruction was that they should report upon the mood accompanying or infusing their images; and that (if this procedure were of any help to them) they should

compare images of different types, in order that the moods should be intensified 'by contrast.' The instruction to compare the images set up, in both observers, a tendency to the alternation of imagination and memory, though this tendency was oftentimes cut across by the intrinsic suggestion of the stimulus. The results were similar for the two observers, and need not be separated.

Of 103 imaginations, 72% involve some mood of surprise. This was variously described as wonder, novelty, strangeness, queerness, unusualness, weirdness, fantasticalness, creepiness, peculiar discomfort, and lack of ordinariness. The imaginations not thus accompanied were almost always images of commonplace objects, which had been a part of the ordinary mental furniture of the observer; that is, they were habitual imaginations.

There were 154 memories. In many cases, the feelings and emotions connected with the remembered event were so strong that it was hardly possible to determine whether any affective coloring attached to the image as image. In all other cases, both observers found a recognitive mood; and both expressed the opinion, at the end of the series of observations, that a glimpse of this mood could be caught, by alert introspection, at some stage or other in the course of every memory image.

We may say then that, under the conditions of our experiments, surprise or novelty is the characteristic mood in imagining, and recognition or familiarity the characteristic mood in remembering. On the question of relative intensity, the observers thought that the mood of imagination appeared the stronger simply because it is not interfered with by other emotions or moods. The image of imagination is new and strange, and that—so far as our experiments go—is all; the image of memory is familiar, but it is also, as a rule, affectively colored in its own right. So the recognitive mood is likely to be swamped. When, however, the memory was indifferent, it seemed to be as strong as the mood of surprise in the corresponding imaginative consciousnesses. And had we succeeded in arousing images of imagination in which the observer took an inventor's pride, or which inspired him with disgust, then (our observers thought) novelty would have weakened to the ordinary level of familiarity.

We attempted no analysis of the moods of surprise and recognition; the conditions were evidently unfavorable to such work, and we desired only to find out what was present, in the imaginative and memorial consciousnesses, that could be considered as a characteristic affective formation. The mood of recognition is genetically, in all probability, a very degenerate form of the emotion of relief, of fear unfulfilled (Titchener, Primer of Psychology, 1900, 191). The mood of surprise, of novelty, of strangeness is apparently a weakened form

of our instinctive fear of the unknown; it shows its derivation, with what was to us an unexpected plainness, in its unpleasant nature: the observers speak of creepiness, weirdness, peculiar discomfort.

Instances of the suppression of novelty by another mood (pleasure in the completed image, charm) will be given later. It is probable that the generally unpleasant character of the mood of novelty, in the records of the present experiments, was due in part to the fact that the observers were dealing, not with a continuous imagination, but with single, detached images, and in part to the fact that these images were suggested to them by words coming in from the outside rather than by the course of their own consciousnesses: *cf.* Titchener, *op. cit.*, 207.

§IV. The Image of Memory and the Image of Imagination Compared

We have now to discuss in detail the introspective differences between the image of imagination and the image of memory. The initials *P* and *V*, in the following paragraphs, refer to the observers in Experiment X (daylight images). For other observations we rely mainly upon Experiment VII (dark room images).

1. Fixation

Imagination. Both *P* and *V* noted, without instruction given, that fixation was necessary if an image was to appear at all. *V* could never move her image, though she could duplicate it at some other point on the wall after renewed fixation. She reported that she could see the original image in its original place, by indirect vision, while she held the new image in direct vision.[1] *P* thought that he could move his

Both *P* and *V*, again, found that the image nearly always image, but was in fact never able to follow it in passage, and said of his own accord that he was not sure it was not a new image, built up after renewed fixation.[2]

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[1] *Cf.* Moore: *op. cit.*, 302.

[2] All of our observers who could obtain images of visual imagination expressed their belief, when questioned, that they could move their images at will; we have noticed an instance under Exp. II. It seems, from the experience of *P* and *V*, that such movement is more probably a shift of fixation followed by the development of a new image; but the point demands further investigation. *Cf.* E. B. Huey, Psychology and Pedagogy of Reading, 1908, 32 ff.

P found that his images invariably tilted in position when he tilted his head; with great effort of attention he could sometimes right them. The same observer, who suffered from imbalance of the ocular muscles, was at times unable, towards the end of the experimental sitting, to hold a steady fixation; his images then 'rolled around' or 'turned somersaults.'

Both observers reported that they could follow the lines (*e. g.*, the curved lines of a coiled snake) within the image, as one can follow the lines in a stereographic perception.

remained unchanged as long as it was visible at all; like a perception, it stood still to be scrutinized, though it yielded no more detail than at the first glance. Occasionally, when the first appearance of the image was incomplete or otherwise unsatisfactory, transformations would be observed in the defective parts, or the entire image would flicker out, and a new one would take its place.[1]

Usually the image was on, or at the exact distance of, the wall. Sometimes, especially for *V*, it was nearer, and induced a "cross-eye feeling"; the point of fixation could then be roughly determined by passing the finger back and forth through the image and noting double-images. If the object imaged were too large for the field of vision at the distance of the wall, the perception of distance was rather uncomfortably disturbed; the image was 'felt' to be at the distance of the wall, but was of a size that the imaged object should have had at a greater distance. The wall, under these conditions, dropped out of consciousnes, or remained only to color the atmosphere surrounding the image.

A series of 20 tests gave the average duration of *V*'s images as about 90 sec. The maximal duration noted was nearly 180 sec. If an effort was made to hold the image to the bitter end, it grew smaller and less distinct, receded in space, and finally faded out. Otherwise, it simply ceased to be there.

We may add the general statement that both *P* and *V* frequently reported absence of bodily movement, and lack of diffused kinæsthetic sensations, especially after comparing these images with the images of memory.

Memory. These images did not involve a steady fixation, but rather a definite eye-movement; the observer was conscious of seeking them in a definite direction.[2] The image appeared with a determinate orientation to the observer, which might, however, differ widely from that of the original experience. When once the image had appeared, the observer could very rarely, and then only with the greatest effort of attention, get it with any other orientation; if he succeeded, the image

[1] Several experimenters have reported oscillation or fluctuation of the image. *V* sometimes noticed fluctuations, at first of a period of some 40 sec., and then of increasing frequency. Fluctuation seems to vary not only with the individual, but also with the condition of the individual. *Cf.* Moore: *op. cit.*, 295.

Both observers found that the image flickered with winking, and that if the eyes were closed it usually disappeared or, at best, remained for a very short time.

[2] This point has been noticed by many investigators: Fechner, Kuhlmann, Meakin, Moore, Murray, Slaughter.

was a 'ghost image,' barely perceptible and very difficult to hold. Neither *P* nor *V* was able to move the image.[1]

The image was fleeting and instable; it could not be deliberately scrutinized; it stayed for 10 seconds at most. The strong tendency was to move incessantly from part to part of the image-content.[2]

As a rule, the images appeared to be some distance away, often approximating the distance of the remembered object from the observer at the time of observation.

The images involved a good deal of general kinæsthesis. *V* could see the pail, which on the remembered occasion she swung in her hand, only by holding her arm up before her eyes as she sought to form the image. So far as introspection could determine, the movements involved in memory were weaker duplicates of those involved in the remembered experience.[3]

2. Visual Characters

Imagination. The images were apt to take on the illumination of their surroundings. In the dark room, they were dusky, apparently illuminated by the phosphorescent fixation spot. Sometimes this spot became an integral part of the image: a candle in a window, a lantern in a cave, etc. In the daylight they were much lighter; closing the eyes darkened them, and tinged them with red. They often took on a fringe or halo of the color of the wall.

V incidentally noted a distinct after-image succeeding the image of a fire. After this observation, *P* and *V* made special tests of the arousal of after-images. *P* was never certain that he obtained an after-image, though at times he thought he had a vague negative image. *V* obtained several fairly distinct negative after-images.

On certain occasions, when the image was very near, *V* tried the effect of alternate monocular observations, and found that the image changed precisely as perception changes.

Memory. The images did not take on the illumination of their surroundings; even in the dark room they retained their daylight character, if the originals had been daylight experiences.[4] The fixation spot in the dark room never became

[1] *P* found that it did not alter its position when he tilted his head. The image did not, for either observer, flicker with winking, nor did closing the eyes banish it.

[2] *Cf.* Murray: *op. cit.*, 231; Slaughter: *op. cit.*, 531.

[3] *Cf.* G. M. Whipple: this *Journal*, xiii, 1902, 259 f.

[4] We had no objective control of the dullness or brightness of these images; for that, recourse must be had to simpler modes of image (Bentley, this *Journal*, xi, 1899, 42 f.). The point here is that the imaginations were dark in the dark and light in the light, while the

a part of the image. In the moderate diffused daylight of the laboratory, the observers felt themselves blinking in the glare of a remembered sunny path. Illumination was not affected by closing the eyes. There was no fringe from the color of the wall. No after-image was obtained from a memory image.

The visual imagery was scrappy and fleeting. The observer would be aware that there were large blanks in the visual picture, but the instant he turned his attention to any particular blank it was no longer empty, but filled by a bit of imagery. Trial was made in vain to trace the boundaries of the blanks, or of their visual fillings. There was a great deal in consciousness (organic, kinæsthetic processes) which the observer could convert at any moment into visual contents. The visual contents themselves were filmy; 'colorless etchings,' 'rough outlines;' they fused so intimately with the entire mental complex that the observer was forced, again and again, to remark "I don't know what I see and what I don't see." The images of imagination were oftentimes, on the contrary, very highly colored.

3. Affective and Organic Elements

Mood. The mood of imagination is, as we have seen, that of surprise (unfamiliarity, novelty, strangeness); the mood of memory is, patently, the recognitive mood. In the case of habitual imaginations, of what one of our observers termed 'generic images,' there was either no affective setting or a very weak mood of familiarity.

We were especially interested to discover feelings of reality (or unreality) and activity (or passivity). Both types of consciousness usually excluded any thought of the image as an image; the image engrossed the attention, and the experience was real. However, we gained some hints—more could not be expected—of the basis and contents of the feelings mentioned.

In the case of imagination, the observers sometimes remarked that they "felt as if it had been a dream," that they had "lost time." Here was a feeling of unreality. More often they would say: "it seemed more real than reality;" "not so real as the memory, of course, if you consider it as an experience, but more really there, real as shadows are real;" "the imaginary rabbit was more real than the memory rabbit, more rabbity, and more there." Here was a feeling of something like perceptual reality. Again, "it had objective reality; not as if I had anything at all to do with it." Here was a feeling of independent reality, of the detachment of the image from the observer.

The feeling of perceptual reality seemed to be due, directly, to the sensible characters of the image; its intensity, definite color, duration, stability, steady position in a definite plane, suddenness of entrance when looked for, its wholeness and self-containedness as compared with the image of memory. The observer "took it as if it was an ob-

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memories were dark or light, independently of surroundings, according as they pictured darkness or lightness in the original experience.

ject to be looked at," he "stood off and watched it." The feeling of perceptual reality appeared to be the conscious concomitant of the attitude of contemplative vision; steady fixation, with general muscular relaxation. The feelings of unreality and of independent or detached reality were based—the former, it seemed, exclusively, the latter principally—upon the lack of kinæsthesis. The observers often remarked that they "had done nothing about the image," that they had not responded to it as they would have done to a perception. This was one reason for the difficulty of distance localization; not only was the observer aware that he got, *e. g.*, an entire person into the field of vision, as he could not have got a real person at the given distance, but he was also disturbed by the absence of any kinæsthetic cue or bodily reaction of his own, by which he might gauge the distance. The dream-character may, then, be ascribed to the lack of kinæsthesis. The feeling of detached reality is due in part to this lack, and in part to the perceptual character of the image itself.

The feeling of reality that attached to the memory image was of a different kind; it was the feeling of real occurrence in past personal experience. The observers identified it with the recognitive mood, together with the conscious concomitants of movements; movements of adjustment, of response, of imitation. These movements fairly abounded; the observer was not now a spectator of a show, but a responsible participator in the experience.[1]

The conditions were not fitted for anything like an exact analysis. We add only that all observers were clear that the feelings of reality and unreality were complex, and that they would analyze into organic and kinæsthetic factors.

The image of imagination was not often accompanied by a feeling of activity; usually it "just came of its own accord." Sometimes, however, there was a feeling of exhilaration, of enhanced mental activity. On a few occasions (not invariably) we were able to connect this feeling of mental activity with the travelling of the point of regard over the lines of the image, and its 'active' examination in detail. The memory image, also as a rule, came of itself, and was receptively, passively taken; though at times there was a feeling of restless activity, always (so far as we noted) accompanying movements of imitative exploration. But the feeling of activity was not, in these experiments, at all a marked feature either of the imaginative or of the memorial consciousness.

Organic Factors. In the case of imagination we find, besides the organic concomitants of concentrated attention, a good deal of empathic sensation. With an image of a bunch of grapes the observer spoke of "a cool, juicy feeling all over;" with a parrot, of "a feeling of smoothness and softness all over me; not tactual [*i. e.*, not cutaneous];" with a fish, of "cool, pleasant sensations all up my arms; slippery feeling in my throat; coolness in my eyes. The object spreads all over me and I over it; it is not referred to me but I belong to it;" with a bowl, of "organic sensations in chest; cool object in

[1] On one occasion the observer *V* had side by side a pure fancy image and an indefinitely recognized but still distinctly memorial image of memory. She was able to look several times from the one to the other, and to compare both the images themselves and her attitude to them. She reported that "the two moods were strikingly different; it would be impossible ever to bring the two images together; both were real, but it was as if the reality belonged to two different thinking organs."

and against it;" with an apple, of "cool juicy round feeling in myself;" with a rose, of "nothing except sensations of pink; I seem to swim around in it and it went all over me." Introspections of this type were very frequent, though not always as explicit as those quoted. Frequently the observer called out, after the image had run on for a few seconds, "there! I seemed to jump into it; went all over me." Sometimes, however, the organic sensations were present from the first.

While the image of memory brought with it into consciousness many organic factors, these were always of an imitative sort, factors that had been concerned in the original experience. The kinæsthetic and organic elements were, in very many cases, at least as important as the visual imagery itself; we have quoted an observer to the effect that he was doubtful as to what he saw and what came from other cues. The whole organic effect was radically different in the two types of consciousness.

4. Temporal Course

The images of imagination appear more quickly, more suddenly, and more as a whole, than the images of memory. They persist longer, and are far less changeable during their course.

These statements, in their relative form, hold for all observers, in spite of considerable individual variation.

5. State and Arrangement of Consciousness

In imagination, the attention is very narrowly focussed; there is a close resemblance to the hypnotic consciousness. The absence of the large motor responses eliminates much of the ordinary perceptual consciousness. We have as a result great clearness and intensity of the image, its spatial isolation in a surrounding haze, and the affective and organic concomitants noted above.

The word 'charm' was sometimes used to express the feeling of pleasurable excitement which went with this semi-hypnotic state. "It had the charm of a dream; felt like a dream." "It does not feel normal, though it feels real." "It is prettier than a rose ever was," "redder than wine ever was," "greener than any grass," "more brilliant than emeralds," "I feel myself through it as through a fog; the lights and shades help, and give it charm." "It is clear, though in a dim light;" "clear as an image but not as an existence;" "the only thing in consciousness;" "I became the image;" "one part stands out very clear; beyond it shades off into a fog or haze;"[1] "it looks as things look when you turn your head upside down." All these reports point to a high level of attentional clearness.

In memory, on the other hand, attention is wandering and

[1] *Cf.* W. Lay: Mental Imagery, 1898, 3.

diffused. It hurries to a number of diverse factors, and rests on none. Tactual, auditory and visual images combine with organic sensations into an instable flux, sent into many channels by as many different cues. The visual image flits through consciousness, now this and now that feature of it rising to clarity and claiming interest, but all passing so swiftly that introspection is difficult. In general, the visual image is flat and thin, without much perspective or light and shade, and without the peculiar vividness of coloring that we find in the image of imagination. It is so entangled with the self-feelings and with kinæsthetic bits of spatial direction and arrangement, that the observer is at a loss to know what he sees, while he knows that he is conscious of much more than he sees; he is conscious of an underlying fixity of occurrence, of a total setting in which the visual images sink and are lost, of a breadth of context in which he can move at will, converting into visual terms this or that detail, as his interest prompts. All this is given in attitudinal terms, with definite fragments of imagery, or kinæsthesis, or some organic complex, rising in quick succession to the level of clearness.

The patterns of the two consciousnesses are thus essentially different. In imagination there is a permanent narrowing of consciousness, with inhibition of all irrelevant associative material; in memory, consciousness is as it is in the ordinary waking state as contrasted with hypnosis, a formation that now narrows and now broadens, liable to the irruption of any chance association. The great difference between memory and perception derives from the elimination of sensations of special sense, and the consequent predominance of kinæsthetic and organic factors.

Summary

We have used the phrases 'image of memory' and 'image of imagination' to denote, not the elementary image-process that is co-ordinate with sensation, but a complex formation, of the same level as the perception.

(1) We find that, under suitable experimental conditions, a distinctly supraliminal visual perception may be mistaken for and incorporated into an image of imagination, without the least suspicion on the observer's part that any external stimulus is present to the eye. The perception may be of such definiteness that instructed and competent observers, in presence of it, have declared our results 'incredible' and have pronounced the stimulus 'ridiculously real.' Yet there was not one uninstructed observer who discovered the deception for himself. It follows that the image of imagination must have much in common with the perception of everyday life.

(2) For preliminary purposes, images of memory may be distinguished from images of imagination as having particularity and personal reference.

(3) We find that, in the great majority of cases, memory images of sight, sound and smell involve gross movements of eyes, larynx and nostrils, while the corresponding imaginations involve no such movements.

(4) A detailed comparison of visual images of memory and of imagination brings out the following differences: memory involves eye-movement and general kinæsthesis, imagination involves steady fixation and lack of general kinæsthesis; memory images are scrappy, filmy, and give no after-images, while images of imagination are substantial, complete, and sometimes give after-images; the mood of memory is that of familiarity or recognition, intrinsically pleasant, the mood of imagination is that of unfamiliarity or novelty, intrinsically unpleasant; memory implies imitative movement and the correlated organic sensations, imagination implies kinæsthetic and organic empathy; memory images arise more slowly, are more changeable in course, and last less long than images of imagination; memory implies roving attention and a mass of associative material, while imagination involves concentrated and quasi-hypnotic attention with inhibition of associations.

We thus reach the general conclusion that the materials of imagination are closely akin to those of perception. Popular psychology looks upon memory as a photographic record of past experience, and regards imagination as working with kaleidoscopic, instable, undependable materials. Precisely the reverse appears to be true. The image of memory is stable and fixed in meaning, in reference; but it is exceedingly instable as conscious content. The image of imagination is the photographic record, a stable formation that stands still to be looked at. This state of affairs seems, indeed, after the event, natural enough. It is just because the memory image is instable, liable to all sorts of interchange, suppression, short-cutting, substitution, telescoping, that it is psychologically available for memory; that a mass of past experience can be packed into small representative compass. And it is just because the image of imagination is stable and unchanging that it is psychologically available for the artistic purpose, for constructive embodiment. If an image could not decay, we should have but little memory; if an image could not persist, we should have but poor imagination.

It is now a question whether the stability and vividness of the image of imagination are due simply to its kinæsthetic support, to the immobility of the organ of sense, or whether peripheral sensory excitations are actually involved in it.

We incline to the latter view, though we regard the kinæsthetic support as a contributing factor.

Finally, we wish to emphasize the point that our results, positive as they are, hold at present only for the conditions under which they were obtained and for the observers upon whose introspections they rest. Any attempt at generalization would be premature. We have found that imagination is distinguished from memory by sensory vividness; but it may be that this vividness is not essential to imagination, and in any case it does not mark off imagination from pictorial thought. We have not found that imagination is in general more markedly emotive than memory; but, under other circumstances, this may prove to be the case. We have not found that imagination implies a plan, a voluntary synthesis; yet it may do so in other cases. We have dealt only with two complexes, of a low degree of complication, which had the advantage that they were easily manageable, that they were promptly distinguished by our observers, though at first in non-psychological terms, and that their distinction is definitely of the kind that, in the text-books, separates memory from imagination. Even if our results are verified by other investigators, the great bulk of the chapter on the Experimental Psychology of Imagination remains to be written.

COLOR-NAMES OF ENGLISH SCHOOL-CHILDREN[1]

By W. H. WINCH.

The question whether the color sense of man has undergone an appreciable evolution within historic times is one which, after half a century of discussion, still remains open.

The most considerable work on the color sense of primitive peoples known to me is that done on the Cambridge Anthropological Expedition to the Torres Straits, by Dr. W. H. R. Rivers; and that done on the Fillipinos by Dr. R. S. Woodworth.

A detailed account of the work of the former is given in the second volume of the Cambridge Reports on the expedition to Torres Straits, in part two of the volumes on Physiology and Psychology. Using Lovibond's Tintometer, Dr. Rivers found that high thresholds and defective nomenclature went together; that is to say, peoples who had no name for Blue, for example, had a much higher threshold for Blue than Europeans, whereas for Red, for which they had a well-known name, their threshold was much lower. Other converging evidences seemed to show that there was an order of color development which, after 'Light' and 'Dark,' began with Red, and was followed by Yellow, then by Green and, finally, by Blue. There was thus agreement in the main between the order of development as determined by inferences from language and the probable order as determined by "objective" methods.

The results of Professor R. S. Woodworth's work with the Fillipinos are not yet published; it would therefore be wrong to anticipate his method or conclusions; but I gather from a very able review written by him that he has not been led to conclude that the relationship above indicated between language and sensibility is at all a close one, and that, moreover, among the primitive peoples studied by him, there was no especial lack of sensibility to "Blue."

So far, then, there seems to be no knowledge *current* among anthropological experts capable of deciding the question. However, child study—the term is, perhaps, an unhappy one, and many errors have been committed in its name—may have

[1]The substance of this paper was read before the British Psychological Society on Jan. 30, 1904.

been more fortunate than anthropology. For, after all, if ontogeny repeats phylogeny in any way whatever, we need not always argue from phylogeny to ontogeny; we can turn our argument about, and child study can help anthropology.

Do observations on the color-sense of children yield results parallel to those of Dr. Rivers, or of Professor Woodworth, or of neither? Is there any definite indication of development at all? I think there is, but I am very far from feeling that the question is settled.

Professor Preyer in *Die Seele des Kindes*, 1881, records experiments with a single child. The test used appeared to depend on the connection between name and color having been correctly established. There seems some doubt as to what his conclusions were. Dr. Holden and Mr. Bosse, in the *Archives of Ophthalmology* XXIX, 1900, say, in summarizing his work, "the greatest percentage of correct responses were for yellow, then for brown, red, violet, black, rose, orange, gray, green and blue in the order named." Dr. Rivers, in the *Popular Science Monthly* of May, 1901, interprets Preyer's results to mean that the child makes no distinctions of color till the end of the second year, and that 'Red' is distinguished before 'Blue.'

Preyer himself says, in the English translation of his work known as the *Infant Mind*, "I put color tests to my child for a series of years. Before he could talk, he was unable to distinguish Green and Blue as surely as Red and Yellow, and White and Black; and he confounded Green and Blue with Gray, and at a later period he confounded their names."

Binet's results differed from Preyer's; he believed Blue was the first color perceived, but, like Preyer, he experimented on one child only. Binet's experiments, related in the *Revue Philosophique* XXX, p. 583, also depended upon the connection between color and name having been established.

In both cases, however, that of Preyer and that of Binet,[1] there appears to have been a direct inference from a knowledge of the name and the order in which that name was acquired to the order in which the color was perceived.

Professor Mark Baldwin, in his *Mental Development in the Child and in the Race*, page 53, quite rightly criticised the assumption underlying the above arguments, and experimented with one of his children, beginning at the ninth month. He used a method, in which names were not employed, which he called the 'dynamogenic' method. He presented colored papers

[1] I am quite sure, though I cannot find the reference, that later work by Binet makes a clear distinction between the order of perceiving and that of naming.

at different distances, and noticed to which and how much the child moved to get them. He found the order of attractiveness to be Blue, White, Red, Green. Unfortunately, however, the order of attractiveness need not be the order of perception; it is fairly certain from general psychological considerations that it is not; for we know that what is quite well known loses attractiveness, after a time, from the mere fact that it is well known.

Perhaps we may pause for a moment to express surprise, if not consternation, that our search for the order of color development has so far brought us difficulty after dfficulty. We see that the problem has occupied the minds of very able men; yet little agreement, if any, can be found so far.

There is, I think, a general biologic consideration which, had it been present in the minds of those who have argued trenchantly on these facts derived from individual cases, might have saved much logomachy. Ought we to have expected exact uniformity in the development of these children? The presence of the sensory basis of color sensation—whatever that may be—is only one element or factor necessary in the perception of color. One must have exactly similar and constant environments for all observers before one can conclude that the variations found in perception are due to differences in the sensory basis. And even then why should the development of the sensory basis in every individual be quite the same even under exactly similar environments?

We must, I think, be content with something much less definite than early experimenters thought possible—the true result, perhaps, will be a kind of mean from a number of developments which are not quite the same. But I am afraid I am going ahead a little too fast; I am not aware that, up to the present, my reader will think that a case has been made out for any order of development at all; he is more likely to say, having regard to the variety of different orders so far thrust upon his notice, "A plague upon all your orders."

But I must not leave this section of the subject without a reference to the work of Garbini, *Evoluzione del senso cromatica nei bambini. Arch. per l'Antropologia* XXIV. I regret to say that I have been unable to see the original paper; but I am glad to be able to express my acknowledgments of gratitude to the succinct account of it given in Dr. A. F. Chamberlain's valuable compendium, *The Child*.

Garbini employed a name method and a matching method without names, and compared the orders of the two developments—of the order in which the colors were most accurately matched and of the order in which, later in the child's life,

they were most accurately named. He experimented with about 550 children, boys and girls, between the ages of three and six; so that, with his work, we have no feeling of insecurity from the paucity of the cases. Garbini found that the order of perception was identical with the order of naming, and that this order was Red, Green, Yellow, Orange, Blue, Violet. Perhaps it may be found useful subsequently, if I give the Italian names—rosso, verde, giallo, arancio, blu, violetto.

These results are of great importance. We have (1) the fact of the development of the color sense, (2) the order in which the colors become perceptible, (3) the fact of development in color nomenclature, (4) the order in which the names of the colors become known, and (5) the exact correspondence between these two orders of development—all these important questions dealt with and settled in a way which appears, on the face of it, certainly coherent and extremely probable.

And there is very marked resemblance between the results of the work with Garbini's children, the conclusions drawn from the philological consideration of ancient language, and the results of Dr. Rivers's work both philological and experimental.

The anthropologic order, so to speak, is White—Black; Red; Yellow; Green; Blue.

Garbini's paidological order is White—Black; Red; Green; Yellow; Blue.

Garbini concluded that the child begins to see Red and Green towards the end of his second year; and that, during his third year, he begins to discern Yellow, and has the first impression of Orange, Blue and Violet; but only begins, for, from thence onwards until the sixth year and even later, the child only becomes adequately familiar with Orange, Blue and Violet.

There is no doubt that the work on children, so far, had led to the conclusion that the color sense was absent until the period just before the close of the second year, and that its growth was somewhat slow. But it was not doubted that lightness and darkness, whiteness, grayness and blackness were perceptible much earlier; and that objects were discriminated by means of their varying brightnesses before their different colors, as such, assisted the child to distinguish them in any way.

I wish now to refer again to an interesting piece of work by Dr. Holden and Mr. Bosse of New York. The paper may be found in full in the *Archives of Ophthalmology*, Vol. XXIX, No. 2, 1900. They pointed out, truly enough, that a child might reach to a colored object because of its difference in brightness from its background, even when no color could be

seen at all. One might, perhaps, go further and say that a child *might* successfully *match* a number of colored objects, putting all the reds with the reds, all the blues with the blues, and so on, simply because the reds and blues were unequal in point of luminosity.

The importance of this consideration is decidedly great, and it would certainly seem that if, by placing the colors on backgrounds of equal luminosity with themselves, we took away one means of discrimination—admittedly possessed by quite young children—and forced the child to discriminate by means of his color vision alone, we should discover that *the sensibility to color, as distinct from that to brightness, was a very much later development than had previously been supposed.*

Dr. Holden and Mr. Bosse aimed at eliminating this element of luminosity altogether; and found, by their method, that the development of color sensibility was *much earlier* than had previously been supposed. The child had now less to go by, but he discriminated better—a certainly unexpected conclusion. Is it allowable to suggest that, after all, Garbini's children were Italians, and these little ones were among the brightest and least shy inmates of the New York Nurseries?

Perhaps a word or two may be admissible as to the nature of the test. A small square of colored paper was placed on a gray of equal luminosity. "If the young child made an effort to grasp the colored square it must have perceived *that color*," say the authors. Well, I suppose we all grant that the infant did not try to pick up a piece of colored paper which he could not see; but he might see it and not grasp for it, one would think; this, indeed, is allowed by the authors, who say that the failure to reach out for the green, blue and violet might have been in part due to lack of interest in them, though they think that the perception of colors of the red end of the spectrum is acquired a little earlier than of those of shorter wave length. All these colors are seen and reached for within the first year. The method hardly allows the statement of a definite order of perception, but it seems apparent that Red, Orange, Yellow were visible quite early; Green, Blue, Violet were perceived later, though, in some cases, no evidence could be obtained that Blue was perceptible at all.

All this interesting work goes in a general way to support the anthropological view before quoted; but there are some interesting differences between these results and those of Garbini.

Dr. Holden and Mr. Bosse assert that average infants of seven or eight months old promptly react to Red, Orange and Yellow; and that, with infants of ten to twelve months of age, there was prompt reaction to Green, Blue and Violet as well.

Garbini thinks that the first chromatic perceptions occur between the sixteenth and twenty-fourth month; that these first color perceptions are Red and Green; that the child begins to differentiate Yellow during the third year, and then Orange, Blue and Violet in the order named.

The problem may be put thus: Does the child become aware of colors in the spectral order beginning from the red end, or does he find certain non-adjacent portions of the spectrum more distinct as colors at first, and perceive the intermediate and gradation colors afterwards? I put the case in this way merely as an illustration and not because I wish to insinuate that spectral order has necessarily anything to do with the question of the order of color perception in human beings. With all deference to physicists I am strongly of opinion that the construction of a psychological theory of color-development which will stand has not been much advanced by constant reference to physical problems.

Perhaps it may be suggested at this point that, so far, neither of the great color-theories have received much encouragement. It is not unfair to suppose that the so-called primary color substances would have been developed first. Now Garbini's results do seem intelligible on the Hering theory, but the Holden-Bosse results seem hard to understand on either the Young-Helmholtz or the Hering theory, which, by the way, is also the case with what we might call the anthropological order of development.

But—I am afraid there is a but—is the inference, on which Dr. Holden's results are based, sound? May we say, when the child reaches for the red, orange, yellow, green or blue paper that it must have perceived *that* color? It seems to me we should only be justified in saying so if he could distinguish these colors from each other. *Must have perceived color* would perhaps have been a fairer conclusion. This consideration may solve the awkward question as to the great difference in the ages at which American and Italian children appear to perceive the colors. Garbini's children were required to discriminate one color from another; the American children were required only to perceive color, not colors.

It is not uncommon for young children to apply with avidity the same color name to all colors; sometimes that name is Red, sometimes it is Blue, and sometimes it is Color. Now, if we are justified in believing that there is some kind of parallelism between the development of the nomenclature and the development of the sensibility—and, if we accept the results of Garbini with children and Dr. Rivers with primitive peoples, we must so believe—I should like to go a step further than has been done so far. I should like to suggest not only,

as has been said, that the order in which the names are learned is the order in which the sensibilities for the various colors are developed, but that the application of one or two names to all colors is an indication that before Red, Green, Blue and the rest are seen as colors, all these things are seen as color, and at an early stage are neither Red, Green, Blue, Yellow, nor any other sensation with which adults are acquainted, but a unitary sensation out of which certain components become more and more definite as time goes on, and more and more unlike each other; when this latter stage begins we have the perception of colors; it is this stage only which we can speak of as that of color development.

But at this point I shall be told and quite rightly, I fear, that the correlation between sensibility and language is much too doubtful to support such a superstructure of hypothesis. Arguments can be advanced on both sides; on the side of correlation it can be urged

1. That we adults ask names for new thoughts, new things and new sensations when we apprehend them, and that much more is this the case with children.

2. That the objective tests of Dr. Rivers and Garbini, coupled with name tests, exhibit certain correspondences, with Garbini a correspondence of orders of development, with Dr. Rivers a correspondence between a high threshold of perception and defective nomenclature.

An opponent could plead: firstly, that we can perceive plenty of things for which we have no names, that we do not ask names merely because we perceive things but because we are interested in them, and especially that we can see many colors for which we have no names. Mr. Hoopdriver, in Mr. H. G. Wells's *Wheels of Chance*, when for the first time he saw the flowers of the country side, marvelled that they had no names: one cannot pretend that he did not see them because he did not know their names. Secondly, that cases are on evidence, for example, the Nubians examined by Virchow, in which defective language was not accompanied by defective sensibility. Thirdly, that if these persons whose terminology for color is defective are given instruction as to the separate and several names, their color names will be found as accurate as our own, at least, in a little while, for one could hardly expect the names to be acquired without some misplacement of terms at first.

How can we overcome this difficulty as to language? It seems to me that we must find cases *in which certain color names have an equal chance of being known*, so far as instruction is concerned. Then, if some names are found to be known better than others, this may arise from two causes:

1. Because the name is easier to articulate and remember: "yellow," for example, is a hard color-name, "red" is an easy one.

2. Because the sensibility to the color is better developed; for we could hardly expect a child to learn the color name of a color which he can barely discriminate, if at all.

There is a critical consideration sometimes advanced which needs mention. It is urged that the child may discriminate all the spectral colors from one another at a very early age, but that he does not learn the names of all the colors which he can discriminate, but only of those by which he is attracted. In finding the order in which the names become known, one is therefore only finding an order of attractiveness, not an order of perception. This consideration appears to me be a weighty one; and, were the child left to pick up his color names from information which he gets at home about colors which interest him personally, I should attach great importance to it. But the children, whose color names I ask the reader to consider in this paper, are children who learn these things at school. And the important points in this connection are that, in the infant schools which these children attend, colored objects are in use—worsted balls and beads—which are of fairly constant coloration in all schools; and that, of these things, the teachers do not, on the whole, teach the name of one color more frequently than another. If the child can easily discriminate the color, can easily articulate and remember the name, and can connect the name with the color, there is reason to suppose, on the methods of teaching employed, that he would learn the name for every color used, and not merely for those for which he felt a liking.

To me, therefore, it seemed that the conditions were especially favorable for a test with color names, and that we might, under these conditions, draw some inferences from the order of naming to the order of perception. The exercises were two in number:

1. The colored worsted balls of the Froebelian Gift 1 were arranged in the following order: Red, Orange, Yellow, Green, Blue, Violet. Each child was asked separately to name the colors. In some cases the colors were presented in the reverse order.

2. Strings of large glass beads in use in the school, the coloration and luminosity of which differed markedly from those of the worsted balls of corresponding colors, were arranged in the following order: White, Black, Red, *Amber,

*NOTE. The Amber beads were a transparent brownish yellow. It was not to be expected that young children would call them yellow.

TABLE I A

Showing the Color-Names of School "A"

Name	Sex	Age		First Exercise						Second Exercise					
Initial only		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
S. F.		3	3	Black	Black	—	Black	Red	Red	Black	Black	Black	Black	Red	Red
V. G.		3	3	White	—	Yellow-Red	Red	Blue	Yellow	Blue	Red	Red	Red	Red	Red
A. T.		3	3	Black	Black	Black	Red	Black	Black	Red	Black	Red	Black	Red	Red
B. C.		3	4	—	—	—	—	Blue	Blue	Blue	—	Blue	White	—	Blue
F. P.		3	5	Blue	—	Red	Red	Blue	Blue	Blue	Black	Red	Blue	Blue	Red
D. M.		3	11	Red	Red	—	—	—	—	—	Black	Red	—	—	—
C. J.		4	2	Red	Blue	Yellow	Green	Violet	Violet	White	Black	Red	Black	Yellow	Blue
M. P.		4	5	Red	Blue	Red	Dark	Red	Blue	White	Black	White	Blue	Blue	Blue
E. P.		4	6	Red	Pale Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
E. W.		4	7	Red	Dark Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
E. T.		4	7	Red	—	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
L. D.		4	10	Red	Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
N. B.		4	11	Red	Blue	Green	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue

NOTES. 1. When two color-names occur, separated by a hyphen, both names were applied in the order in which they were written.

2. The blank spaces mean that no answer was given. I should have been inclined to regard these blanks as better than errors, had it not been apparent that the older children, even when ignorant of the name which we apply, were almost invariably ready with some name which to them seemed applicable.

3. I regret that I am unable to give the sex of the children in this table.

Green, Blue. In some cases the colors were presented in the reverse order. Each child was asked separately to name the colors. No child proceeded immediately from Exercise 1 to Exercise 2. All the children tested in the same school attempted Exercise 1 first, and then, after a short interval, attempted Exercise 2.

Perhaps a few comments directly attached to the Table may make my subsequent argument clearer.

In the first group of children—whose ages are between 3 and 4—we find only a few color names in use, and these are used with what seems to us an absolute lack of discrimination. It may subsequently prove of importance if we set down in order the number of times each name is used; but, before doing so, it is worth noting that, of these six children, three —S. F., A. T., and D. M.—had *two* color names only, Red and Black. B. C. had two names only, Blue and White. F. P. had three names only, Red, Black and Blue. V. G. had four only, Red, Yellow, Blue and White.

TABLE I B

Showing the number of times each Color-Name was used in School "A"

Color-Names	Ages 3-4	Ages 4-5	All Ages
* Black	16 times	9 times	25 times
White	2 "	8 "	10 "
Red	22 "	15 "	37 "
Blue	13 "	18 "	31 "
Green	0 "	12 "	12 "
Yellow	2 "	12 "	14 "
Orange	0 "	0 "	0 "
Violet	0 "	7 "	7 "
Brown	0 "	2 "	2 "

* NOTE 1. I have included in this number the word "Dark" which was once given for "Green." The word "Black" is unusually prominent in this school. Can this be connected with the fact that the only social events of general interest in this immediate neighborhood are funerals?

NOTE 2. It will be remembered, in considering the figures of Tables marked B, that there were no Blacks and Whites among the worsted balls.

The next school at which the observations were made, School "S", was socially on a level with School 'A'; but its environment was, if anything, more colorless, and without the spice of excitement and the visual stimulation of moving things provided in the former case by proximity to a main road.

TABLE I c

Showing the percentage of Color-Names correctly used in School "A"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	4 out of 6	67	7 out of 7	100
White	0 " " 6	0	7 " " 7	100
Red	5 " " 12	42	13 " " 14	93
Blue	4 " " 12	33	12 " " 14	86
Green	0 " " 12	0	11 " " 14	79
Yellow	1 " " 6	17	5 " " 7	71
Orange	0 " " 6	0	0 " " 7	0
Violet	0 " " 6	0	6 " " 7	86

In the first group of children, ranging in age from 3 years 1 month to 3 years 10 months, we find one child who insists on responding to our questions about the names of colors, by the word 'Color,' varying it only in one case, in which she says 'Another Color.' E. S. somewhat resembles J. K.; she used the word 'Color' twice; in all other cases she used the name 'Black.' F. M., after starting well, lapsed into a color terminology of her own. 'Blue' she called 'E.' in both exercises, though they were very different Blues, and 'Green' she called 'Air,' though they were very different Greens. Three other children fall back on the word 'Color,' or 'Color-one'—F. K., V. C. and H. B. And in the second group of children—those varying from 4 years 2 months to 4 years 9 months—the oldest child is found using the word 'color' not less than three times. The theoretical interest lies in this: We have to ask ourselves how far we are justified in supposing that the word 'color', when used by a young child, represents a concept embracing different units previously discriminated as red, blue, etc.; or whether, prior to this discrimination, 'color' does not mean for him a unitary apprehension. Then the parts of the continuum which stand out earliest will, other conditions being equal, get their names first. This, at least, is a possible explanation of the very early use of the word 'color.' And it is fairly obvious that, on association principles, the word would appear much later; it would need to be generalized from its instances.

The next school at which the observations were taken was in a neighborhood decidedly superior to those of the two previous schools, and the school was very full. It was not easy, therefore, to obtain very young children—the youngest I could get was 3 years 5 months old; and I could only find four

TABLE II A

Showing the Color-Names of School "S"

Name	Sex	Age		First Exercise						Second Exercise					
Initials only		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
F. K.	B	3	1	Red	Red	Yellow	Green	Blue	Blue	White	Color	Red	Blue	Blue	Blue
V. C.	B	3	2	White	Color-one	White	Green	Red	Red	White	White	White	White	White	Blue
E. S.	G	3	5	Black	Black	Black	Color	Black	Color	Black	Black	Black	—	Black	Black
J. K.	G	3	9	Color	Color	Color	Color	Color	Another Color	Color	Color	Color	Color	Color	Color
F. M.	G	3	9	Red	Yellow	Orange	Air	E	Air	Air	E	Air	E	Air	E
H. B.	B	3	10	White	—	—	—	Blue	Blue	—	—	Yellow	—	—	Color
M. McD.	G	3	10	Red	Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Yellow	Blue
E. J.	G	4	2	Red	Brown	Brown	Green	Blue	Violet	White	Color	Red	Green-Brown	Green	Green
W. B.	B	4	6	Red	Brown	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
R. H.	B	4	7	Red	Green	Yellow	Green	Blue	Blue	White	Black	Green	Yellow-Green	Green	Blue
M. M.	G	4	8	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Brown	Green	Blue
W. M.	B	4	9	Blue	White	Color	Green	Blue	Color	White	White	Green	Blue	Color	Blue

TABLE II B

Showing the number of times each Color-Name was used in School "S"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	10 times	3 times	13 times
White	10 "	7 "	17 "
Red	8 "	7 "	15 "
Blue	10 "	13 "	23 "
Green	3 "	15 "	18 "
Yellow	6 "	5 "	11 "
Orange	1 "	0 "	1 "
Violet	1 "	2 "	3 "
Brown	1 "	5 "	6 "

TABLE II C

Showing the Percentage of Color-Names Correctly used in School "S"

	Ages 3-4		Ages 4-5	
Color-Names	Times Correctly used	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	2 out of 7	29	3 out of 5	60
White	3 " " 7	43	5 " " 5	100
Red	5 " " 14	36	7 " " 10	70
Blue	6 " " 14	43	9 " " 10	90
Green	3 " " 14	21	9 " " 10	90
Yellow	2 " " 7	29	3 " " 5	60
Orange	0 " " 7	0	0 " " 5	0
Violet	1 " " 7	14	2 " " 5	40

children then present under 4 years of age: I was, however, able to get an adequate sample of children between 4 and 5.

We note at once an increase in the number of color-names. One child, K. S., on one occasion only, falls back on the word 'Color,' and only one child, D. S., has as few as two color-names. She, it will be noted, used 'Red' and 'White' only. All the other children have at least four names, though they are not, of course, always accurately applied.

The next school at which the observations were made was in a poor neighborhood with a colorless and cheerless environment, though not very far removed from the bustle and movement of a great thoroughfare.

One child, A. N., applied the name 'Blue' to every color indiscriminately; this was perhaps in her language equivalent to the word 'Color.' The latter word was not used by any child in this school; but one child used the word 'grass' for the amber and the green beads in the second exercise. The names

TABLE III A

Showing the Color-Names of School "G"

Name	Sex	Age		First Exercise						Second Exercise					
Initials only		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
D. S.	G	3	5	Red	Red	Red	White	White	Red	Red	White	Red	White	Red	White
H. H.	G	3	5	Red	—	Yellow	Green	Red	Blue	White	Black	Red	Green	Yellow	Blue
K. S.	G	3	8	Red	White	White	Green	Color	Red	White	Black	Red	Red	Green	Red
M. S.	G	3	10	Blue	Blue	White	White	Blue	White	White	Black	Red	White	Blue	Red
E. G.	B	4	2	Red	Yellow	—	Green	Blue	Blue	White	—	Red	Yellow	Green	Blue
G. G.	B	4	4	Red	Brown	Blue	Blue	Blue	Blue	White	Black	Red	Brown	Blue	Blue
E. M.	G	4	5	Red	Orange	Green(1) Yellow(2)	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
K. B.	G	4	6	Red	—	—	Red	Red	Red	White	Black	Pink	Red	Red	Red
V. G.	B	4	6	Red	Yellow	Yellow	Red	Blue	Blue	White	Blue	Red	Yellow	Red	Blue
W. B.	B	4	9	Red	Orange	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
A. H.	G	4	10	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue

TABLE III B

Showing the number of times each Color-Name was used in School "G"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	3 times	5 times	8 times
White	14 "	7 "	21 "
Red	17 "	21 "	38 "
Blue	6 "	22 "	28 "
Green	4 "	9 "	13 "
Yellow	2 "	12 "	14 "
Orange	0 "	2 "	2 "
Violet	0 "	0 "	0 "
Brown	0 "	2 "	2 "

TABLE III C

Showing the Percentage of Color-Names correctly used in School "G"

	Ages 3-4		Ages 4-5	
Color-Names	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	3 out of 4	75	5 out of 7	71
White	3 " " 4	75	7 " " 7	100
Red	7 " " 8	87	13 " " 14	93
Blue	2 " " 8	25	12 " " 14	86
Green	3 " " 8	37	8 " " 14	57
Yellow	1 " " 4	25	4 " " 7	57
Orange	0 " " 4	0	2 " " 7	29
Violet	0 " " 4	0	0 " " 7	0

are generally more developed among these children, though one boy, G. C., has three names only—Red, White Black.

The school at which similar observations were next taken was situated in a better neighborhood, and stood beside a park. The word Green is, undoubtedly, a more prominent feature than usual in the vocabulary of these children, though the average age of both groups, namely that consisting of children from 3 to 4 years of age, and that of children from 4 to 5, is low.

At the commencement of the table given above, we are fortunate in having five very young children, one of whom, C.M., has one color-name only—'Red,' which she applies to *all* colors; it is her word for color. G. B. has two color-names only, 'Blue' and 'Dark.' T. F. has three only, 'Green,' 'Black,' 'White.' S. L. and W.H. have three names only, 'Red,' 'Green,' 'White.' E. H., who is several months older, has three names only, 'Red,' 'Black' and 'Green.' The great

TABLE IV A

Showing the Color-Names of School "R"

Name		Age		First Exercise						Second Exercise					
Initials only	Sex	yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
A. N.	G	3	1	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue
G. C.	G	3	3	Red	Red	White	Red	White	Red	White	Black	Red	White	Green	White
G. C.	B	3	5	Red	White	White	Red	White	White	White	Black	White	Red	White	White
B. B.	B	3	8	Yellow	—	—	—	Blue	Blue	White	Black	Red	Grass	Grass	Blue
M. H.	G	3	8	Blue	White	Yellow	Blue	White	Blue	White	Black	Black	White	Black	Blue
J. C.	G	3	8	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Black	Green	Green	Blue
A. P.	G	3	11	Black	White	—	Green	White	Violet	Green	Black	—	—	Green	Blue
E. N.	G	4	3	Red	—	—	Green	Blue	Blue	White	Black	Red	—	Green	Blue
C. B.	G	4	3	Red	White	White	Blue	Red	Green	White	Blue	Red	—	Gay	—
B. C.	B	4	5	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Brown	Green	Blue
F. B.	G	4	7	Red	Yellow	White	Green	Blue	Blue	White	Black	Red	—	Green	Blue
L. W.	G	4	11	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Brown	Green	Blue
M. C.	G	4	11	Red	Brown	Yellow	Blue	Blue	Blue	White	Black	Red	Yellow	Blue	Blue

TABLE IV B

Showing the number of Times each Color-Name was used in School "R"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	10 times	5 times	15 times
White	21 "	9 "	30 "
Red	10 "	13 "	23 "
Blue	23 "	19 "	42 "
Green	7 "	9 "	16 "
Yellow	4 "	7 "	11 "
Orange	0 "	0 "	0 "
Violet	1 "	0 "	1 "
Brown	0 "	3 "	3 "

TABLE IV C

Showing the Percentage of Color-Names correctly used in School "R"

	Ages 3-4		Ages 4-5	
Color-Names	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	6 out of 7	86	5 out of 6	83
White	5 " " 7	71	6 " " 6	100
Red	5 " " 14	36	12 " " 12	100
Blue	8 " " 14	57	10 " " 12	83
Green	5 " " 14	36	8 " " 12	67
Yellow	2 " " 7	29	3 " " 6	50
Orange	0 " " 7	0	0 " " 6	0
Violet	1 " " 7	14	0 " " 6	0

feature of interest in this table is the very early appearance of 'Green.' It is noteworthy that the elder group were much better acquainted than usual with names like 'Violet,' 'Orange,' 'Brown,' so that the earlier appearance of 'Green' may not be due to environment, as I suggested in a previous similar case, but to the slightly superior social class of the children.

The school in which the next observations were made is situated in a somewhat poor district ; but one in which the small house and long garden, once characteristic of London's poorer suburbs, have not been entirely swept away by modern improvements!

The two youngest children, C. N. and W. N., were very deficient in color names, as was L. P., who was six months older—an important difference at this age, so far as the acquisition of language is concerned. L. P. is another case to be added to those which we have already found, in which the

TABLE V A

Showing the Color-Names of School "P"

Name		Age		First Exercise						Second Exercise					
Initials only	Sex	yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
G. B.	B	3	0	Blue	Blue	Blue	Dark	Blue	Dark	Blue	Blue	Blue	Dark	Blue	Blue
T. F.	G	3	1	White	—	—	Green	White	White	White	Black	White	White	Green	White
S. L.	B	3	1	Red	White	White	Green	—	Red	—	Red	—	Red	White	White
W. H.	B	3	2	Green	Red	Green	Red	Green	Green	White	Red	White	White	White	Green
C. M.	G	3	2	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red
E. H.	B	3	6	Red	Green	Green	Red	Red	Red	—	Black	Red	—	Red	Red
T. B.	B	3	7	Red	Red	Like butter	Green	Blue	Blue	White	Black	—	Green	Green	Blue
C. M.	G	3	7	Red	Yellow	—	Green	Blue	Blue	—	Black	Red	—	—	Blue
L. B.	G	4	3	Red	Orange	White	Green	Blue	Blue	White	Black	Red	Brown	Green	Blue
H. O.	B	4	3	Red	Yellow	—	Green	Blue	Violet	White	Black	Red	—	Green	Blue
A. C.	B	4	6	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
N. H.	G	4	7	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
C. M.	B	4	8	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
A. H.	B	4	8	Red	—	White	Green	Blue	Blue	White	Black	Red	Green	Green	Blue

TABLE V B

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	4 times	6 times	10 times
White	16 "	8 "	24 "
Red	30 "	12 "	42 "
Blue	15 "	14 "	29 "
Green	14 "	13 "	27 "
Yellow	1 "	4 "	5 "
Orange	0 "	4 "	4 "
Violet	0 "	4 "	4 "
Brown	0 "	4 "	4 "

TABLE V C

Showing the Percentage of Color-Names correctly used in School "P"

	Ages 3-4		Ages 4-5	
Color-Names	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	4 out of 8	50	6 out of 6	100
White	3 " " 8	37	6 " " 6	100
Red	8 " " 16	50	12 " " 12	100
Blue	6 " " 16	37	12 " " 12	100
Green	6 " " 16	37	12 " " 12	100
Yellow	0 " " 8	0	3 " " 6	50
Orange	0 " " 8	0	4 " " 6	67
Violet	0 " " 8	0	4 " " 6	67

word 'Red' is the generic color-name; C. N. has the word 'White' as well. W. N., though tried in a variety of ways, beyond those specified in the exercises, was not found to be the possessor of any color-name at all; he spoke quite freely but used no color words which his teachers or I could recognize or even see any constancy in as applied to colors.

The most striking feature in this table is the complete absence of the word 'Blue' in the younger group, and its infrequency as compared with 'Green' in both groups. These facts should be noted since they are, I think, quite exceptional as far as these tables are concerned.

The next observations presented were made at a school situated in a decidedly better neighborhood than the two preceding ones. It stands facing a large open space with many trees, much grass, and some flowers.

From one child, T. W., we failed altogether to elicit any words which appeared to have reference to color; but generally speaking there was decided superiority in color vocabulary in this school. 'Green,' again, as in the previous case in

TABLE VI A

Showing the Color-Names of School "C"

Name		Age		First Exercise						Second Exercise					
Initial only	Sex	yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
C. N.	B	3	1	White	White	White	White	—	—	Red	Red	Red	—	White	White
W. N.	B	3	1	—	—	—	—	—	—	—	—	—	—	—	—
L. P.	G	3	7	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red
B. H.	G	3	8	Red	White	—	Green	Red	Red	Red	Black	Red	White	Green	Red
L. S.	G	4	0	Red	—	Blue	Green	White	—	White	Red	Red	—	Green	—
J. P.	G	4	8	Red	Brown	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
T. A.	B	4	8	Red	Violet	—	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
N. S.	G	4	9	Red	Violet	Yellow	Green	Blue	Violet	White	Black	Red	Violet	Blue	Violet
W. N.	B	4	10	Red	—	Yellow	Green	Violet	Violet	White	Black	Red	Yellow	Green	Blue

TABLE VI B

Showing the number of times each Color-Name was used in School "C"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	1 time	4 times	5 times
White	10 times	4 "	14 "
Red	24 "	8 "	32 "
Blue	1 "	7 "	8 "
Green	4 "	7 "	11 "
Yellow	0 "	6 "	6 "
Orange	0 "	0 "	0 "
Violet	0 "	9 "	9 "
Brown	0 "	1 "	1 "

TABLE VI C

Showing the Percentage of Color-Names correctly used in School "C"

	Ages 3-4		Ages 4-5	
Color-Names	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	1 out of 5	20	4 out of 4	100
White	1 " " 5	20	4 " " 4	100
Red	7 " " 10	70	8 " " 8	100
Blue	0 " " 10	0	6 " " 8	75
Green	4 " " 10	40	7 " " 8	87
Yellow	0 " " 5	0	3 " " 4	75
Orange	0 " " 5	0	0 " " 4	0
Violet	0 " " 5	0	4 " " 4	100

which the school fronted a park, was better known than usual. Only one other child, H. T., had less than four color-names, and he had 'White,' 'Black' and 'Red.'

It is impossible to read the foregoing tables without coming to the conclusion that, both in the younger and older groups, there is much agreement as to the order of the accuracy with which the color-names are applied. The names Black, White and Red are used with approximately equal accuracy by both these groups of children who, as I have said, have been taught equally to name all these colors. There can, too, I think, be little doubt that Blue occupies the next place in order of accuracy, that Green follows Blue, and is, in its turn, followed by Yellow, whilst Violet and Orange are quite at the bottom of the list.

It must not be supposed, however, that we are quite sure that every *one* of these children had been taught exactly equally every *one* of these color-names. The three-year-old group in school

TABLE VII A

Showing the Color-Names of School "D"

Name		Age		First Exercise						Second Exercise					
Initial only	Sex	yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
T. S.	B	3	1	White	—	White	Green	White	White	White	Black	Green	White	Red	Green
T. W.	B	3	2	—	—	—	—	—	—	—	—	—	—	—	—
C. H.	B	3	3	Green	Yellow	—	Green	Blue	Blue	White	Black	Red	Green	—	—
H. T.	B	3	6	White	White	Red	Red	Red	Red	White	Black	White	Red	Red	Red
E. H.	G	3	8	Red	Red	—	Green	—	—	White	Black	Red	—	Green	Blue
D. P.	G	3	8	Red	—	Yellow	Green	Blue	Violet	White	Black	Red	—	Red	Blue
L. P.*†	B	3	7	—	Red	—	Red	Pink	Red	Blue	Dirty-Blue	—	Blue	Beads of White	Blue
L. S.*	B	3	7	Red	Red	Yellow-Red	Blue	Blue	Blue	Wash	Blown	Red	Red	Green	Blue
E. H.*	B	3	7	Green	Green	White	Green	White	Green	White	Black	Green	Green	Green	Blue
A. D.*	B	3	8	Red	Blue	White	Red	Blue	Color	White	Black	Blue	Color	Blue	Blue
T. E.	B	4	0	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
M. F.	G	4	2	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
T. W.	B	4	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
M. B.	G	4	5	Red	Orange	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
R. D.	B	4	7	Red	Yellow	Yellow	—	Green	Violet	White	Black	Red	Yellow	Green	Blue
W. W.	G	4	8	Red	Orange	Green	Green	Yellow	Orange	White	Red	Red	Yellow	Green	Blue

* In these four cases the balls and beads were presented in the reverse of the usual order.
† L. P. volunteered the information that the blue beads were like the red beads.

TABLE VII B

Showing the number of times each Color-Name was used in School "D"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	7 times	5 times	12 times
White	19 "	6 "	25 "
Red	25 "	13 "	38 "
Blue	19 "	11 "	30 "
Green	17 "	13 "	30 "
Yellow	3 "	13 "	16 "
Orange	0 "	6 "	6 "
Violet	1 "	4 "	5 "
Brown	1 "	0 "	1 "

TABLE VII C

Showing the Percentage of Color-Names Correctly used in School "D"

	Ages 3-4		Ages 4-5	
Color-Names	Names used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	7 out of 10	70	5 out of 6	83
White	7 " " 10	70	6 " " 6	100
Red	8 " " 20	40	12 " " 12	100
Blue	10 " " 20	50	10 " " 12	83
Green	8 " " 20	40	11 " " 12	92
Yellow	2 " " 10	20	5 " " 6	83
Orange	0 " " 10	0	5 " " 6	83
Violet	1 " " 10	10	4 " " 6	67

TABLE VIII

Summary, Showing the Percentage of Color-Names Correctly used in all Schools.

Color-Names	Ages 3-4								Ages 4-5							
	School A	School S	School G	School R	School P	School C	School D	Average	School A	School S	School G	School R	School P	School C	School D	Average
Black	67	29	75	86	50	20	70	57	100	60	71	83	100	100	83	85
White	0	43	75	71	37	20	70	45	100	100	100	100	100	100	100	100
Red	42	36	87	36	50	70	40	52	93	70	93	100	100	100	100	94
Blue	33	43	25	57	37	0	50	35	86	90	86	83	100	75	83	86
Green	0	21	37	36	37	40	40	30	79	90	57	67	100	87	92	82
Yellow	17	29	25	29	0	0	20	17	71	60	57	50	50	75	83	64
Orange	0	0	0	0	0	0	0	0	0	0	29	0	67	0	83	26
Violet	0	14	0	14	0	0	10	5	86	40	0	0	67	100	67	51

'C,' for example, had done no previous work with the beads; hence, probably, their low mark for Black and White, for there were *no* blacks or whites among the worsted balls; and in the case of Blue, half at least of the chances of correct naming were absent also. I am quite unable to explain why, in the three-year-old group in School 'A,' the names White and Green should not have been accurately applied even once, for it is quite certain that the green ball of the Froebelian Gifts had been used. But, despite some anomalies, we may lay claim, on the whole, to have made out the order of accuracy with which the names are applied; and when we remember that there are neither black nor white balls in the Froebelian Gift 1, with which most of the early teaching of color is done, we shall probably accept the order of the correct application of color-names as

Black — White: Red: Blue: Green: Yellow: Violet: Orange.

But is it supposed that we are able from this order to infer a similar order in the development of the child's sensibilities to various colors? Let us deal for a moment with what is *not* asserted. It is *not* asserted that because a child cannot apply the word White correctly that he has no sensibility to White. It is *not* asserted that because a child says Red when looking at Green that we therefrom may infer that the child sees no difference whatever between Red and Green. I have no doubt at all, both from my own observations and from the work of others, that the discrimination of colors is a much earlier thing than the correct naming of them. The naming follows the sensibility and it follows *longo intervallo*, let us agree on that. But is the order in which the names are acquired, *provided that the child has equal opportunities of learning them and connecting them with the appropriate colors*, an indication of the order in which the sensibility to the different colors was acquired or became developed?

Garbini maintained not only that it was, but that he had shown, by separate tests on sensibility and vocabulary, that, for Italian children, the two orders coincided. The color-names became known in the order:

Rosso, Verde, Giallo, Arancio, Blu, Violetto, and this is exactly the order, according to Garbini, in which, with younger children, the sensibilities to these various colors were developed.

If Garbini's order coincided with my own, I should doubtless feel additional confidence in our common opinion that the *name-order* may indicate a *sensation-order*. I do not *know* that Garbini's children had had, generally speaking, equal opportunities of becoming acquainted with all the names. If they

had, as is quite possible, we must conclude either, 1. That the Italian order of sensational development is different from our own; or, 2. That the difficulties of learning the Italian names do not correspond to the difficulties in learning our English color-names.

If the former proposition be true, it seems that we must each rely wholly on national researches, for it is hard to believe in an Italian order without believing also in a German order, and in a French order, and, perhaps, in an American order. Personally, I should find this variety of orders hard to accept; but my reader must decide for himself.

If the second proposition be true, we *might* get a satisfactory explanation of the difference found; but it hardly seems that Italians would find *giallo*, pronounced djah'loh, so much easier than our children find *yellow*, that with them yellow would become known third and with us sixth. Blue is not a particularly easy word for a young English child, yet 'blue' with us comes second, and with Italian children the corresponding word 'blu' is learnt fifth.[1]

Let us turn for a moment to corresponding work in German. I do not propose to give any detailed account of individual researches; but from the admirable and comprehensive summary given by Professor Meumann in his *Experimental Pedagogy*,[2] we learn that the order in which color-names become known to German children is as follows:

Weiss—Schwarz: Rot: Blau; Grün: Gelb: Violett-Orange.

Now this is exactly the order in which the names become known to London children: We are more inclined to doubt a specific Italian order after this, unless we can show that the difficulties in the German vocabulary coincide with the difficulties in our vocabulary, whereas the Italian articulatory difficulties do not. Obviously, however, we cannot show this. Our word Yellow is a hard one for our children, Gelb is easy for German children, yet Yellow is learnt sixth in both countries. The difficult German word is Grün. Any one who has ever heard German teachers laboriously, and somewhat unsuccessfully, teaching the pronunciation of words containing the modified 'u' will have a very present sense of the difficulty of the word Grün. On the other hand 'green' is for English children a word of moderate difficulty only—yet green is learnt fifth in both countries. Rot and Red are of similar difficulty: Blau and Blue, Weiss and White, Violett and Violet, Orange and Orange, are of similar difficulty; and it does not surprise us to

[1] This argument fails if the children are taught the word 'azzuro' for 'blue.'

[2] Einführung in die Experimentelle Pädagogik, Vol. 1, page 108.

see them occupy exactly similar positions in both countries. But in Schwarz, Grün and Gelb we have words by no means corresponding in difficulty of pronunciation to Black, Green and Yellow, and yet *the same color is named in the same order of accuracy in Germany and England.*

It certainly does appear that there is in operation here a common factor which is strong enough to overcome the difference in order which may have been produced by the differences in the difficulty of the names.

Perhaps, at this juncture, it might be well to refer to a criticism on the method which was urged by Professor Sully during a discussion on the Color Sense at a meeting of the Anthropological Society. The children, he urged, would not have had equal chances of learning the color-names, because some of them would be taught the names at home. I fear, however, that these Elementary School children would not, in scarcely any case, receive instruction in color naming at home. They might have, and probably had, picked up something from hearing the talk of the elder members of the family. But the vital point is that the color preferences[1] of their elder brothers and sisters and fathers and mothers, and hence the colors they would hear most about are not such as would lead to knowledge in the order White—Black: Red: Blue: Green: Yellow. I do not wish to exclude the home talk as an altogether irrelevant factor, but I incline *à priori* to the view that it is of little weight in this case, and from the results it is apparently not operative.

During the course of these observations I learnt much, both positively and negatively, from the experienced teachers who were kind enough to assist me. When they discovered that I was best pleased with the child who was *trying* to name rightly, but who could not yet succeed, and that the boy or girl who rattled them all off glibly did not arouse my interest, they began to look at the matter in an objective spirit (it is fair to the teachers to say that I was then an Inspector of Schools) and took much interest in the experiments. Several of them, who, during the first exercise, heard the children confidently applying the wrong names, said to me: "Oh, they don't know, and are just saying anything." I might have answered that they were not exactly saying *anything*, they were, in almost every case, confining themselves to color-names. That limitation, by itself, counted for something. And when we came to the second exercise and found the child consistent in error, as he often was, the teachers gave

[1] Winch: Color Preferences of School Children. *British Journal of Psychology*, Dec., 1909.

up that explanation, especially as they saw that, both in luminosity and color, the beads of the second exercise differed much from the worsted balls of the first exercise.

Yet I am by no means indisposed to admit that some naming occurred which seemed guided by little else than an indefinite association between all color sensations and all known color names. Professor Preyer, in the *Infant Mind*, page 13, in my judgment, overworks this factor. He says, referring to the way in which children call colors by their wrong names:

"They know the words which name colors, and they have the sensations of color all right, but they do not know which words and colors belong together. They do not understand their own sensations of color." Let us hope the last sentence is a mistranslation, for it is irrelevant to the issue, if not misleading—do any of us *understand* our own sensations of color? Professor Preyer's explanation, which resembles, in another form, the teachers' explanation—that the children were just saying anything—admits of a simple verification. I urge that it is not wholly satisfactory.

If there are no connections between the names and the particular sensations, but only between color and color-names, of which *any of the latter may be given to any of the former until the right one is learnt*, then the errors made should be scattered over the colors equally. If 'Red' is not known rightly in connection with its name, it will be called Green, Blue, Yellow an equal number of times, or, at least, approximately so, provided the child can say these names and knows as, *ex-hypothesi*, he does, that they *are* color names. Let us see, by reference to our tables, how the errors are distributed. How many times, for example, is Red called Green, how many times is it called Yellow, and so on?

Red	was	named	White	13	times
"	"	"	Black	8	"
"	"	"	Blue	9	"
"	"	"	Green	7	"
"	"	"	Yellow	2	"
Blue	was	named	White	15	"
"	"	"	Black	3	"
"	"	"	Red	23	"
"	"	"	Green	5	"
"	"	"	Yellow	1	"
Green	was	named	White	9	"
"	"	"	Black	3	"
"	"	"	Red	26	"
"	"	"	Blue	16	"
"	"	"	Yellow	3	"

Yellow	was	named	White	28	"
"	"	"	Black	4	"
"	"	"	Red	16	"
"	"	"	Blue	8	"
"	"	"	Green	10	"

As it was decidedly unfair to count the answers to the questions as to the color of the transparent brownish-yellow or amber beads as involving knowledge or ignorance of yellow, the number of answers given as to the color of the yellow worsted ball is doubled, so as to compare with those given for Red, Blue, and Green.

If any one of my readers cares to work out the distribution of these errors among the children of various ages and various schools, he will find that the totals are fairly representative of all the children, and are not mere haphazard totals. That being the case, we may safely draw the conclusion that some general tendencies are at work; there is not a chance distribution of error over all the color names. There appears to be no tendency to use the term Yellow; this would be intelligible on the hypothesis that the word is very much harder to learn and to say than the other color names. But this hypothesis would not explain why the color is very frequently called 'White,' less frequently 'Red,' sometimes 'Blue' and 'Green,' and scarcely ever 'Black.' It staggers us a little to see that it is called 'Blue' as many times as it is, *though only about as many as would be found on a chance distribution.* Especially are we surprised, since Blue is only once called 'Yellow.'

The green ball was a dark green; the green beads were a light green; we might, therefore, have been disposed to think that the mistakes, 'White' and 'Black,' would be equal in number; but there is, except for 'Red,' no considerable number of errors in which the name 'Black' is used. Green seems to be called in error 'Red,' 'Blue,' and White. It seems good to us adults to call green 'Blue,' especially the dark green; nor does it seem unintelligible to us to call a light green 'White'; but to call green 'Red,' as was done no less than 26 times, seems to us sheer perversion. Would it be quite unreasonable to suppose that there are many cases in which, for children, the term 'Red' embraces all color sensation from the red end of the spectrum up to, and including, the green?

The errors for Blue present some expected results and some difficulties. There is no tendency to confuse with Black, as is decidedly the case with primitive peoples. There are no confusions with Yellow; we should not expect any. The term 'White,' however, is not more frequently applied to the light beads than to the dark blue worsted balls. The outstanding

point is the confusion with Red. Perhaps it is explained thus: 'Red' is the first-color name, Blue is the second; does Red then stand at first rather for color than for Red as we know it? If early color sensation is a much more unitary thing than with us adults, such a condition of nomenclature is not impossible. Adults would expect many confusions of Blue with Green, but there are only five, that is, less than a merely chance number if the errors were scattered equally over all the well-known color names.

Again in the errors for Red there are some difficulties. It is hard to see why the dark red worsted ball should be called 'White,' as it often was, and the light red beads called 'Black,' as they sometimes were. One might fall back on the theory of chance distribution for the Black, Blue and Green; but such a theory would hardly apply to the errors of White and Yellow. 'Blue' is the second color-name acquired and we should perhaps expect that it may, in some cases, even precede 'Red'; for there are certainly considerable individual differences. There are a few confusions with Green, but nothing like the number we should expect if the sensations Green and Red were as closely alike as might appear from the large number of times which Green was called 'Red.' Green we might say is called 'Red,' but Red is not called 'Green.'

Green, of course, is the third color-name in order of acquisition, and hence we might simply regard the errors as the tendency of a well-known name to spread beyond its primary application. The errors could very largely be explained in that way were it not for the small number of times which the word 'Black' is used. Only with the Red does the error 'Black' approximate to its chance number; in all other cases, though a well-known and correctly-applied name, it is not a name given to other colors, whereas the very reverse is the case with 'White.' There are evidently factors operating here which give a distribution of error much unlike what would be found on the solution that the children *just say anything*, if they do not know the right name. They *appear* to be sensationally guided; the names *appear* to be attached or in process of attachment. I do not wish to deny that the child's manner sometimes leads us to believe that he will attach any color name to any color percept, nor that there is, in these cases, a chance distribution of error. But, that another factor is working also, the analysis is sufficient proof.

What is that factor? I suggest that we only need to apply a certain principle a little lower down in the evolutionary scale which we already apply higher up. A person calls Violet 'Blue' or Orange 'Yellow'; or at night he calls Green 'Blue.' "That's very natural," we say; meaning that these colors

look to us so similar that we are not surprised that people sometimes call them by the same name. We should not call Red by the names 'Blue' or 'Green'; nor should we call Blue and Green by the name 'Red.' But children do. Shall we suppose, therefore, that these colors look so much more alike to them that they see no incongruity in using the same name; just as most of us who are not botanists are accustomed to group together all the lovely varieties of the grasses under one undistinguishing term "Grass"?

Would it be hopelessly rash to suggest that color sensation is at first unitary, that it differentiates in the order Red, Blue, Green, Yellow, Violet, Orange? As each part, if this word is applicable, of the *continuum* becomes distinct, it tends as a growing power always does, sensational or conceptional, to be seen as covering too much, and a period of contraction follows: we know what it is and more than it is, and then we know what it is not. By-and-by, if these color 'units' receive names in the order in which they have themselves been differentiated from the color *continuum*, we can, by testing for the names at a later period, find out the order of sensational differentiation at an earlier period.

Obviously, why not try a method, as Garbini did, for testing the color sensations of young children as well as for testing the application of their color names? There will be difficulties here in getting sufficient numbers of unselected children; I mean accidentally selected, of course; using the word accident in its logical sense. On this work I have been employed, on and off, for a year or two, and hope to present the results some day. My purpose, however, in this paper, is to show what may perhaps be inferred from the color names of children who have had approximately equal sensational and verbal environments.

PRACTICE IN THE CASE OF ADDITION.

By EDWARD L. THORNDIKE, Teachers College, Columbia University.

The experiment reported here was designed to secure information concerning only the amount and rate of improvement and the value of the practice experiment as a method for school work. The practice was not continued long enough nor taken under uniform enough conditions to justify inferences concerning changes in the rate of improvement; and I shall make no attempt to analyze out the factors producing the improvement.

The experiment consisted in adding daily for seven days forty-eight columns each of ten numbers (no 1's or 0's being included). Seven printed blanks had been arranged of equal difficulty.[1] The forty-eight sums were written. The time required was recorded in seconds. The subjects were nineteen university students—eight men and eleven women.

The time taken and the number of examples wrong for each set for each of the nineteen subjects are recorded in Table I. Table II repeats Table I with an addition of one per cent. of the time for forty-eight examples for each example wrong. That is, I estimate that half the time for one example is a just allowance to balance its inaccuracy. This system of allowance is, of course, arbitrary, but it will not prejudice any of the conclusions which I shall draw. They would be the same by any reasonable allowance. Table III summarizes conviently the facts as to the amount and rate of improvement, and its relation to initial ability.

Taking the whole group together, improvement in speed and in accuracy are about equal, the median reduction in time regardless of errors being 31 per cent. and the median reduction in errors regardless of time being 29 per cent. By the scores with allowance for errors the median improvement in general efficiency in addition is 33 per cent. The average improvement is 29 per cent. This is for less than one hour of practice (about fifty-three-minutes).

The individuals vary from cases making no improvement (F and G) to a case of nearly fifty per cent. improvement (K).

That the practice represented by only 2,592 additions made by an educated adult whose addition associations have been long established and often used should produce an improvement of three-tenths, bears witness to the continued plasticity or educability of the synapses involved. It also supports the contention that the degree of efficiency shown by persons in any intellectual function is a result chiefly of specific training in it or the elements of it and only slightly of the transfer to it of the effects of training other functions. If the general training of from fifteen to twenty-five years of a scholarly life were responsible for a large fraction of one's efficiency in "quickness of association" or "accuracy in response," one would not by so little specific training be able to improve so much.

The amount of improvement in this experiment may also add to our confidence that the method of the practice experiment wherein

[1] The improvement is measured from the average of series 1 to the average of series 7, that is, over 6 X 48 examples, each involving nine additions.

one works at one's limit and competes with one's own past record may well be made a regular feature in many school drills. Even if the same length of time produced in children a percentile improvement only half as great as here, the gain would still probably be far greater than the gain by any of the customary forms of drill.

TABLE I

Gives Scores in the Seven Successive Practice Periods

Individual	Sex	Day of beginning	Intervals between tests Hours	First		Second		Third		Fourth		Fifth		Sixth		Seventh	
				Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong
A	m	M	24	550	3	530	5	420	3	420	4	430	4	402	2	382	4
B	m	T	24	620	2	420	4	325	6	420	7	360	9	360	13	345	5
C	m		not reported	560	4	570	3	545	4	515	4	480	1	455	4	470	2
D	m	W	24 except 48 between 4-5	482	18	470	11	420	6	360	9	420	8	300	9	302	8
E	m	W	24 except 48 between 4-5	1020	3	810	3	770	4	680	5	735	6	650	7	605	1
F	m	W	24 except 48 between 4-5	800	3	710	2	715	2	700	3	695	1	655	4	785	3
G	m		not reported	600	0	560	0	510	0	465	0	465	4	468	4	455	0
H	m	T	24 except 72 between 4-5	456	5	390	4	325	3	329	3	368	7	320	3	310	2
I	f	W	24 except 72 between 3-4	420	2	360	0	450	0	450	1	450	1	480	4	470	4
J	f	W	24 except 48 between 4-5	641	3	630	5	608	3	640	5	550	1	545	1	475	0
K	f	W	24 except 48 between 4-5	460	0	375	1	362	0	307	0	297	3	243	2	240	1
L	f	T	24 except 48 between 4-5	560	8	520	3	540	3	440	3	500	0	390	3	385	5
M	f	T	24 except 48 between 5-6	590	4	420	3	360	0	350	1	345	0	330	0	360	0
N	f	T	24 except 48 between 5-6	750	8	740	2	605	1	600	3	650	5	550	4	500	6
O	f	W	24 except 48 between 2-3	420	5	680	1	470	1	495	0	435	3	434	3	410	0
P	f		not reported	870	14	840	11	795	13	630	16	610	7	600	5	530	10
Q	f	W	24 except 48 between 4-5	840	4	750	1	690	1	675	0	700	1	645	0	630	0
R	f	W	24 except 48 between 4-5	690	2	625	3	580	1	510	3	560	3	555	3	470	3
S	f		not reported	866	2	680	1	725	1	595	3	590	1	580	2	520	2

TABLE II

Scores Reduced to Single Variables by Allowance for Examples wrong

Individuals	Sex	First	Second	Third	Fourth	Fifth	Sixth	Seventh
A	m	565	555	432	436	446	410	398
B	m	632	436	345	448	390	405	362
C	m	582	587	567	536	485	473	479
D	m	570	520	445	392	454	327	326
E	m	1050	834	800	714	780	695	611
F	m	824	724	729	721	702	681	808
G	m	600	560	510	465	483	486	455
H	m	479	406	335	339	393	330	316
I	f	428	360	450	455	455	499	489
J	f	660	662	626	672	555	550	475
K	f	460	379	362	307	306	248	240
L	f	606	535	556	453	500	402	404
M	f	614	432	360	354	345	330	360
N	f	810	755	611	618	670	572	530
O	f	441	687	475	495	448	447	410
P	f	992	932	897	730	653	630	583
Q	f	874	758	697	675	707	645	630
R	f	707	644	586	525	577	571	484
S	f	883	687	732	613	596	592	530

The relation of the amount of improvement to initial ability in any practice experiment is of great interest because it gives evidence bearing upon the fundamental problem of the relative shares of original nature and environment in determining the achievements of men.

It has been shown that in the case of educated adults the relative (that is, percentile) differences amongst educated adults in the ability to multiply mentally a three-place number by a three-place number are left unreduced by submitting all the individuals to equal practice.

The differences amongst individuals in the ability to add seem to be due in larger measure to differences in environmental influence. For equal practice does here reduce a little the relative or percentile differences within our group. This will be seen by comparing the relative variability of the group in the seventh practice period with that in the first, or by calculating the co-efficient of correlation between initial ability and percentile improvement. The proportions for highest and lowest individuals, next to highest and next to lowest are:

	1st 19th	2nd 18th	3rd 17th	4th 16th	5th 15th
In first practice period	2.45	2.25	1.92	1.82	1.46
In second practice period	3.36	1.99	1.87	1.62	1.46

The correlation between initial ability and percentile improvement is negative, roughly—¼.

There is, of course, no essential conflict between this result for addition and the opposite result for mental multiplication with two

three-place numbers. The same theoretical view which would expect mental span and ability to manage very complex relationships in a given field to be increased by practice in close dependence upon original capacity, would expect particular associative habits such as thinking of thirteen upon seeing 4, 7 and 2 in a column, to be increased by practice in less close dependence upon original capacity.

TABLE III

The Amount of Improvement in Relation to Initial Ability

Individual	Sex	Initial Ability	Gross Improvement	Percentile Improvement
A	m	565	167	.30
B	m	632	270	.43
C	m	582	103	.18
D	m	570	244	.43
E	m	1050	439	.42
F	m	824	16	.02
G	m	600	145	.24
H	m	479	163	.34
I	f	428	61 (loss)	.14 (loss)
J	f	660	185	.28
K	f	460	220	.48
L	f	606	202	.33
M	f	614	254	.41
N	f	810	280	.35
O	f	441	31	.07
P	f	992	409	.41
Q	f	874	244	.28
R	f	707	223	.31
S	f	883	353	.40
Medians,		614		33
Averages,		674		29

The improvements recorded are of the seventh set of 48 ten-figure examples over the first such set. They represent approximately the practice effect of 2,192 additions, or of from 30 to 75 minutes work.

THE RELATION BETWEEN MEMORY FOR WORDS AND MEMORY FOR NUMBERS, AND THE RELATION BETWEEN MEMORY OVER SHORT AND MEMORY OVER LONG INTERVALS.

By EDWARD L. THORNDIKE, Teachers College, Columbia University.

Measurements of mental relationships are so important and so scanty that I venture to report certain ones in the case of memory, although they are by no means satisfactory with respect to method. They will not, however, be misleading to any one who bears in mind their limitations.

The measurements are of the relations in educated adults: — (1) between (a) the ability to remember a list of twelve words from a single hearing, at a rate of approximately one per second, long enough to write them immediately at the close of the reading; and (b) the same ability in the case of a list of five three-place numbers. There were five lists of (a) and five lists of (b). (2) Between: (a) and (c) the ability to remember the sixty words given in the five tests of (a) twenty-four hours later.

No requirements were made as to the order except, of course, that the order of the digits within each three-place number must be correct. The basis of the memory of (c) was not only the single hearing, and the experience of writing down such words as the individual remembered, but also the experience of scoring one's results from a complete list given to the individual for that purpose.

(1) and (2) do not, that is, measure the relationships in general, but the relationships as influenced by the restriction of the tests to one half-hour in the case of (1) and (2) and the relationship as influenced by the variations in the degree of attention given to the words in scoring results in the case of (2). Moreover, I have not corrected the results for spurious correlation due to sex, nor for attenuation due to the small number of tests.

The lists used and the method of scoring were as follows: The number of individuals was 38 for relation 1 and 40 for relation 2.

The lists of words used were:

near	bell	break	maze	yet
out	cloud	call	cress	shall
false	box	sleep	hob	and
lot	slate	drop	zest	lest
gift	cap	smile	eke	how
end	wing	run	slink	could
cheat	flag	eat	fob	though
thought	bed	cry	lush	when
lose	stone	drink	elk	let
add	pig	hit	bland	your
queer	house	sing	tweak	since
full	nose	skip	lilt	more

The lists of numbers used were:

791	254	639	948	579
469	624	716	851	356

918	683	532	746	823
493	321	228	264	974
671	572	787	435	358

The score for one correct word was 1.

A word apparently misheard but remembered as heard (*e.g.*, slake for slate or amaze for maze) was scored correct. Each individual's testimony was accepted in such cases. For each word written that was not in the list a discount of one word was made. Such errors are rare, making only 3 per cent. of the words written; 50 seconds were allowed to write out the words remembered for each list.

Each three-place number recalled exactly counted 1. Each number of which two digits were correct and correctly placed counted .5. 30 seconds were allowed to write out the numbers for each remembered list.

The obtained 'raw' correlation for (1) is .4½ ± .1. The mixture of the two sexes and the testing of the two traits in the same hour tend to make this higher than the relation between the general ability to remember word lists and the general ability to remember three-place number lists. On the other hand, there is the attenuation due to the variation, in both (a) and (b), of the result from five tests from the person's true ability. I estimate that correction for all three would result in a correlation of about .5½. The relation between (a) and memory of lists of 12 single digits was found to be .6, eight independent records of each being used. Correction for attenuation raises this to .7—. So, until more adequate measures are made, we may accept as the most likely fact that, in such a test of brief retention, a variation in the content from words to numbers reduces the correlation from 1 to about ⅝. Even if the reduction should prove to be to only ¼, the fact would still be very strong evidence of the dependence of efficiency of memory upon content and of the specialization of mental functions in general.

The obtained 'raw' correlation for (2) is .5½ ± .1 Allowing for the mixture of the sexes, the inaccuracies of the original measures, and the individual variations in the experiences upon which the memories for twenty-four hours were based, I estimate the relation as .8 ± .1. I know of no other measure of the relation between brief and long retention in the case of unconnected material. Henderson, in the case of connected trains of thought, gives data for memory over a few minutes from three minutes' study and memory of the same material after forty-eight hours, based upon the three minutes' study and the experience of writing out what was remembered at its close. The resulting correlations would seem, if corrected for attenuation on the one hand, and for mixture of the sexes and of differently selected groups on the other, to be about .9.

The relation between retention of the effects of an experience for one or two minutes and their retention for one or two days thus seems to be one of the closest yet measured in human nature.

PROFESSOR WIRTH ON THE EXPERIMENTAL ANALYSIS OF CONSCIOUSNESS.[1]

By L. R. GEISSLER.

The purpose of Professor Wirth's "Die experimentelle Analyse der Bewusstseinsphänomene," as set forth in the Preface, is to give an account of the present status of those part-problems of experimental psychology which Wundt had already formulated for the first time in 1874. This may account, to some extent, for the author's close adherence to the Wundtian system of psychology, a brief résumé of which is offered in the Introduction and the First Part of the book.

The Introduction deals with "Consciousness as a Natural Individual Unity" and begins with a brief sketch of "The Historical Development of the Concept." It then defines and discusses, in somewhat abstract and argumentative fashion, such points as self-consciousness, psychical causality, psychophysical parallelism, and mental dispositions, and concludes with an outline of "The Internal Organization of Consciousness and the Chief Kinds of Mental Contents." Here we meet with the familiar Wundtian dichotomy of *Vorstellungen* and *Gemütsbewegungen* as representing the objective and subjective phases of consciousness. The latter, again, are subdivided into states of passivity or *Gefühle*, with their positive and negative aspects, and into states of activity or *Willenserlebnisse*, with their blind or impulsive and their rational or voluntary aspects. Toward the end of this general outline (p. 17) the author says: "the concept of conscious content is in itself so general as to include the special characteristics of the higher mental functions, such as representation of an object, etc.; and since, in particular, the concrete content is never entirely void of the immediately experienced character of activity, it does not seem necessary to make any special distinction between 'contents' and 'acts.'" He thus takes sharp issue with one of the fundamental positions of the school of Brentano.

The First Part has for its subject-matter "General Considerations Concerning the Nature of Conscious Phenomena." The author informs us here that he means by experimental psychological analysis the attempt "to find, as far as possible, exactly measurable stimuli and unequivocally predetermined voluntary mental attitudes, as the basis for a universally comparable situation of consciousness" (p. 21). The conscious phenomena in the narrower sense, to which, if I understand him rightly, he intends to confine his analysis, are certain interrelations between concurring spatial and temporal complexes and their qualitative and quantitative aspects. The analysis of these phenomena would, however, be incomplete without considering their possible changes in degrees of consciousness. A complete psychological analysis is thus concerned, on the one hand, with a numerical or qualitative determination of the "Enge des

[1] *Die experimentelle Analyse der Bewusstseinsphänomene*, von W. WIRTH, etatmausserord. Professor der Philosophie und Mitdirektor des Instituts für experimentelle Psychologie in Leipzig. Mit 27 Abbildungen im Text und auf einer Tafel. Braunschweig, Druck und Verlag von F. Vieweg und Sohn. 1908, pp. xiv, 449. Price Mk. 11.

Bewusstseins," and on the other hand, with a quantitative measurement of the degree of consciousness of simultaneous processes.[1] With regard to the latter problem, the author thinks that an immediate or direct estimation of the degree of consciousness by introspection is *a priori* doomed to failure, and for two reasons. First, the range of possible degrees of consciousness of a given mental process, with the one of its extremes bordering unconsciousness, "is something purely intensive, and therefore involves all the difficulties of the method of what is called supraliminal differences for intensities" (p. 33). The second and more important objection is the inconstancy of degree of consciousness in succeeding mental states, which destroys all hope of a successful application of the direct method. The actual reason, however, for Wirth's refusal to consider this possibility more in detail seems to the reviewer to be the fact that it had not received a place in the Wundtian programme. At any rate, recent experiments by the present writer with the method of direct estimation of degrees of clearness have shown that Wirth's *a priori* objections do not hold in actual practice. Their rebuttal in this place may therefore be omitted.

In speaking of degrees of consciousness it must be noted that the author distinguishes them sharply from degrees of clearness. "One may best understand the nature of the degree of consciousness by comparing the direct sense-perceptions with the reproduced ideas of memory and imagination referring to like objects" (pp. 34 f.). The difference between them lies in *Lebhaftigkeit* and *Frische*, vividness and freshness. The reproduced processes involve, as a rule, a much greater range of vividness and freshness, though in direct sense-perception, especially in the briefer processes, various degrees of vividness reveal themselves in the stages "des An- und Absteigens der Empfindungsfrische" (p. 35). In the case of the *Gemütsbewegungen*, this vividness manifests itself with great directness "as the real actuality of consciousness" (p. 36). Even so, the author does not commit himself to saying that degree of vividness and freshness is identical with degree of consciousness; they merely "stand in closest relationship to each other" (p. 36). In general, "the degree of consciousness stands so definitely at the centre of all psychical causal developments, that the significance of an element or of any abstract characteristic is always dependent upon it" (p. 36).

In our immediate experience the degree of consciousness reveals its effects in several ways. "The most immediate consequence of a high degree of consciousness in the case of a concrete idea consists primarily in the clear and distinct differentiation of the idea as a whole from the other mental contents, and of its parts and characteristics from one another, while a lower degree of consciousness corresponds to the opposite, an obscure intermingling and confusion" (p. 36). This clearness-effect is much less marked with emotional contents, as they are "very closely fused in content, and less clear than other equally vivid elements can be that are more sharply differentiated from their surroundings" (p. 37). But even in the case of ideational contents, changes in degree of consciousness are only approximately proportional to changes in clearness. Another important consequence of a high degree of consciousness manifests itself in introspection and memory. The degree of consciousness has a determining influence, *e. g.*, upon the duration "des Abklingens eines Inhaltes"

[1] "Das fertige quantitative Resultat der exacten Analyse wird sich also vorläufig immer aus der Angabe der inhaltlichen Basis in ihren oben beigezogenen psychophysischen Massen einerseits und des an jeder Stelle herrschenden Bewusstseinsgrades andererseits zusammensetzen" (p. 32).

(p. 39), or upon the psychophysical disposition for the reproduction of contents or of connection between contents, or upon the certainty and objective correctness of the various reproduced elements, their characteristics and relations. Finally, in a similar way, the range of symptomatic movements will, under simplified secondary conditions, correspond to the degree of consciousness of the feelings expressed by them, just as the course of external voluntary movements will be related to the degree of consciousness of the involved voluntary activity.

Now in every state of consciousness a purposive voluntary activity, a "zielbewusste Willenstätigkeit" (p. 41), is present in one or the other of its various forms. Attention, *e. g.*, is a form of voluntary activity whose complex of impulses aims at the greater clearness of certain ideational contents, especially of immediately perceived objects and events. Comparison, as another function of voluntary activity, "seeks to increase the degree of consciousness of a qualitative relation between contents" (p. 42). A general scientific term for all these forms is found in Wundt's concept of apperception. Under certain conditions, it is true, the apperceptive function of attention may fail to accomplish its aim in establishing a certain clearness-relievo, but it does not therefore lose its character as a voluntary activity. Under certain other conditions, again, certain mental contents may reach a very high degree of consciousness without any voluntary effort, but only on account of a momentarily high excitability of their underlying psychophysical dispositions. The nature and combinations of the particular impulses employed by apperception, and especially attention, for the sake of clarifying mental contents, depend entirely upon the special nature of the contents themselves. On the whole, these impulses work for an exact positive and negative (or inhibitory) adjustment of the body, and especially of the sense-organ involved. The impulses functioning in reproductive apperception manifest themselves, *e. g.*, in expressive movements and in symbolic tendencies of articulation, of writing, etc., but mainly in the internal apprehension and fixation of perceptions and thought-contents. Although an analytic study of the apperceptive activities can trace these impulses separately with more or less definiteness, yet in any concrete experience they co-operate as a whole, with more or less ease according to degree of practice, so that they may lead to a constant level of maximal apperceptive practice which is of cardinal importance for quantitative results in experiments on the degree of consciousness.

These are the general principles and considerations, according to which the author intends to conduct his analysis of conscious phenomena in the concrete, and which serve as his guide in the selection and treatment of the experimental literature. The rest of the work, from p. 56 to p. 443, is in the main a grand review—a connected presentation and a consistent interpretation in the light of the preceding discussion—of certain groups of psychological and related experiments. They are classified in Wundtian fashion as experiments by the impression-method, treated in the Second Part (pp. 56-340), and experiments by the expression-method, taken in the wider sense to include the study of symptomatic, ergographic, and reaction movements, treated in the Third Part (pp. 341-443). A list of nearly 400 names and authors referred to completes the work.

The Second Part discusses in its first half "The Analysis of a Single Moment," and in its second half "The Course of Continuous Conscious Events and the Time-Perception."

Several methods have been developed for the study of a momentary

consciousness. The first is based upon that effect of a high degree of consciousness which manifests itself in retrospection and memory. It employs the simultaneous exposure of several homogeneous stimuli, especially of the visual, tactual, and auditory kind, and presupposes the dual division of a mental state into focus and background, into a high level of relatively clear, and a low level of relatively obscure, mental processes. The problem is identical with the first part of the purpose of a complete psychological analysis, as described above, namely, the numerical or qualitative determination of the range of consciousness and, in particular, of attention; it has been attempted mainly for visual, tactual, and auditory sensations. The author goes into great detail with regard to the temporal relations of the stimulus-exposures, and his refreshing objectivity of treatment contrasts favorably with the abstract and generalizing language of the preceding introductory chapters.

The second method employed in the analysis of a momentary consciousness is called by Wirth the *Schwellenmethode* or the method of the difference-limen. It is based upon the other effect of a high degree of consciousness, namely, upon the clear and distinct differentiation of a given mental process as a whole from other simultaneous contents, and of its essential features or characteristics from each other. The clearness-effect is most nearly proportional to its cause, the underlying degree of consciousness, in the case of the singling out of a particular mental content from its simultaneous and successive neighbors in the same total perceptive complex. This condition is realized, in its most elementary form, in the determination of the difference-limen for a single characteristic, such as magnitude, position, brightness, etc. The unit for measuring the degree of consciousness of any of these characteristics is found in "the objective difference which evokes a judgment of difference of a determinate clearness and certainty" (p. 100).

In order to put this point more clearly, we may follow up one of Wirth's concrete examples. In a uniformly illuminated, monocular field of vision certain places or regions are so arranged that their brightness may be gradually and measurably increased until they are supraliminally different from the rest of the field. The whole unitary field is now exposed for a certain length of time and observed with a predetermined distribution of attention, *e.g.*, with attention to the right half of the whole field, but with fixation held always upon the same point at the centre. After a convenient interval, which is determined by the observer's attentive *Einstellung* or preparation, the field is briefly re-exposed with a certain change in the brightness of some (to the observer unknown) region. The change is gradually enhanced until it becomes just noticeable. It then constitutes the difference-limen for the region in question, and as such is the reciprocal value of the degree of consciousness pertaining to this particular region. A systematic application of the method, under various experimental conditions, and with distribution of attention either to the different characteristics of the total complex or to its various part-contents, requires that the observers be always able to give a constant amount of attention to the whole complex during both exposures without expecting a change in any particular direction. Even then it is possible that the various part-contents or characteristics depend, for their degree of consciousness, upon secondary phenomena; in vision, *e.g.*, upon the peculiarities of the periphery of the retina. It is therefore further necessary, in most cases, to determine the absolute difference-limen for every characteristic or every part-content with maximal attention to it itself, and thus to obtain

what is called the normal value of the limen. This is then compared with the difference-limen found with a given distribution of attention, and the ratio between the two values constitutes a more exact measure of the degree of consciousness for the particular characteristic or part-content in the original perceptive complex. The ratios thus found, for the various phenomena investigated, are reduced to the same base by making each normal value equal 1, so that the smaller ratios or fractions express the higher degrees of consciousness, and conversely.

The actual experimental application of such a method involves many details and complications, according to the sense-department and the nature of the perceptive complex selected. The field most extensively studied in this way is vision. Wirth discusses at some length the investigation of Mittenzwey, who worked out the degrees of consciousness for the characteristics of magnitude, position, and brightness of one and of several small white circles within a large gray circular field. In the case of one variable circle, with attention equally distributed over all three characteristics, Mittenzwey found that, if measured by the ratio between the normal value and the difference-limen, brightness required the highest degree of consciousness, .54, position demanded less, .67, and magnitude as expressed by decrease, .75, by increase, 1.00. This result shows that, in spite of the observers' intention to distribute their attention equally over all three characteristics, their effort is influenced by the relations existing between the phenomena observed. In the case of several small circles, with distributed attention, Mittenzwey found the following values expressing degrees of consciousness: .37 for magnitude, .36 for position, and .58 for brightness. If the observer concentrated his attention upon the same characteristic in all small circles, then the other characteristics did not suffer much loss in degree of consciousness; while, if all characteristics in one circle were attended to, the corresponding values for the other circles rose considerably, and in some cases their degree of consciousness was practically zero. An extensive distribution of attention shows, therefore, even in the case of one variable characteristic, the greater variety of degrees of consciousness. Its study was accordingly made the aim of Wirth's own experiments with the *Schwellenmethode*, which he very briefly reviews and summarizes. His perceptive complex was the uniformly illuminated field of vision of the left eye. It was divided into 37 regions, which were grouped concentrically around the point of fixation, and could be gradually and measurably increased in brightness. Since visual sensations change very rapidly in degree of clearness during the first 30σ of their rise to consciousness (*Anklingen*), it was only necessary to know the time-interval at which an increase became just noticeable, and to use this as an equivalent of the visual difference-limen. Hence the formula for the calculation of the degree of consciousness pertaining to any one of the 37 regions was:

$$\text{D. Cs.} = \frac{\text{Intensity of j. n. R-increment} \times \text{Duration of R}}{\text{Constant Initial Intensity of Region}}.$$

In accordance with this formula the whole left field of vision was investigated, with the centre as constant fixation-point, but with attention distributed in the following different ways: (*a*) to the whole field, (*b*) to the left half of the field, (*c*) to the upper left quadrant, (*d*) to a region in the left upper periphery, and (*e*) to the fixation-point itself. Unfortunately, the results with these different distributions were obtained at various stages of practice, so that they are not comparable. Other obvious disadvantages of the method are the difficulty of maintaining a com-

plicated and constant *Einstellung* of attention which requires more than three months practice even for a skilled observer (p. 128), and the consequent difficulty of multiplying results with other observers and with variations of conditions. The main result of all these experiments is, according to Wirth, that the monocular field of vision shows at any moment only relatively small differences in clearness. In particular, under the most natural conditions, when the fixation-point is also the point of attention, the difference of average clearness for the five concentric zones is hardly appreciable. The average values, from centre to periphery, are: 1.16, 1.23, 1.23, 1.14, and 1.26. Wirth also applied his *Schwellenmethode* to touch, the pressure being varied intensively by means of von Frey's limen gauge. He used six pressure stimuli on the lower right and left arms, on the backs of the hands, and on the upper surfaces of the feet, and the obtained values were: for the right side 1.56, 1.96, and 1.10, and for the left side 1.36, 1.51, and 1.35. For tonal pitches he employed the *C* of 128 vs. with its first four overtones, all 5 tones produced by electric tuning forks. Their intensification was produced by movement of a noiseless cock in the tube conducting the tone to the ear. The values expressing degrees of consciousness for the four partials *C*, *c*, *g*, and c^1 were, for one observer, 1.31, 1.10, 1.16, and 1.16, and for another observer 1.16, 1.12, 1.14, and 1.02. In both instances, attention was distributed over the whole tonal complex.

In this connection Wirth devotes a few pages to the discussion of the method of measuring attention by distracting stimuli, and especially to its application by Peters, whose results, he thinks, are in general and essential agreement with his own.

Another and much more indirect way of determining degree of clearness makes use of its relation to accidental and constant errors of observation and comparison. (It will be noticed with some surprise that in the heading of this sub-division,[1] as well as in various passages throughout the text,[2] Wirth seems practically to identify degree of consciousness with degree of clearness.) Owing to the relative inconsistency of the psychophysical relation between stimulus and sensation, a given stimulus-difference may, under certain unfavorable conditions, remain subliminal, or a given stimulus equivalence may arouse a consciousness of inequality. The range of such accidental variations, or the mean error, is greatly reduced by a correct and "geschicktes Verhalten" (p. 151) of attention. To maintain such an attitude is, however, difficult, especially in the case of liminal determinations with distributed attention, where the characteristic whose limen is to be determined offers a constant point of attraction for attention. At the same time, expectation easily assimilates those characteristics most closely related to the one under observation, in favor of the clearness of the latter. Mittenzwey found, *e. g.*, mutually supporting assimilations between the characteristics lighter-larger-higher, and darker-smaller-deeper. Such interrelations occur most readily in the obscurer regions of consciousness.

Besides these accidental errors in liminal measurements there occur constant errors of assimilation which may be due, *e. g.*, to intentional distraction of attention during momentary exposures of objects compared, or to the associative influences of ideas. These last are especially strong in the illusions of perception, which there-

[1] "Die indirekte Bestimmung des Klarheitsgrades durch seine Beziehung zu den Beobachtungs- und Vergleichsfehlern," p. 147.

[2] *Cf.* "Denn nach allen früheren Ausführungen stehen *Bewusstseinsgrade oder Klarheit* eines Inhaltes und Richtigkeit seiner repräsentativen Funktionen immer in enger Beziehung zueinander," p. 151. (Italics are mine.)

fore lead most frequently to wrong judgments, that is, to judgments of inequality with equal stimuli, and to judgments of equality with unequal stimuli. The extent to which these judgments depend upon the degree of consciousness of the secondary factors involved may be most easily determined, experimentally, by means of the optical illusions. It has been shown, *e. g.*, by Schumann, who made tachistoscopic exposures of the illusory complexes, and by Einthoven, who employed instantaneous illumination in order to eliminate eye-movements, that a lower degree of consciousness (conditioning what has been called an imperfect "apperceptive accommodation") exaggerates the Müller-Lyer arrow-illusion to a considerable extent. A similar effect can be obtained with longer exposures, according to Benussi, by a voluntary control of apperception. Benussi has further shown that, after practice, an apperceptive emphasis or disregard of these secondary factors increases or decreases the illusory effect. The apperceptive control of secondary factors is paralleled by the phenomena of fixation and of conscious, impulsive or voluntary eye-movements, whose influence is particularly shown in the illusions with depth-perception. Analogous phenomena occur with tactual illusions, which, on the whole, require no new principle of explanation. If, *e. g.*, attention is voluntarily directed first upon the place on the skin toward which the hand is moving, and then upon the various successive positions or attitudes of the moving limb, the constant error involved in equating distances or in localizing points upon the skin may become actually twice as large as if the attention is directed upon the act as a whole, so that each co-operating secondary factor can attain only to such a degree of consciousness as is necessary for a fairly correct judgment. The same principle is involved in the familiar phenomenon of breaking up an habitual action by attending to one of its components, a procedure which disturbs the clearness-distribution of the various contents; so long as they remain on a somewhat low, but more or less uniform level of degree of consciousness, their co-operation as members of the same complex is undisturbed. In this connection Wirth also discusses the temporal displacement of two simultaneous or almost simultaneous stimuli due to variously distributed attention. These phenomena may justly be called reversible illusions of the temporal order, and as such may be said to constitute a parallel to the reversible illusions of space-perception. Here again it has been found, by Hamlin, that a well distributed attention or "alert indifference" insures the largest number of correct judgments. One of the secondary factors here involved, but in Wirth's opinion not yet unambiguously described, is the previous voluntary *Einstellung* or vivid anticipatory imagination of either of the two possible temporal orders, or more particularly the rhythmical exaggeration of the imagined interval between the two stimuli. He thinks that a closer study of this factor may, *e. g.*, give a satisfactory explanation of the fact mentioned by Weyer, that out of the 12 possible combinations in pairs of light sound and touch, the two sequences light-touch and light-sound deviated from the general rule governing the magnitude of the difference-limen for temporal displacement.

This completes the discussion of the three methods used in the study of a momentary consciousness. It must, however, be added that clearness-distribution also depends, to some extent, upon the nature of the mental contents themselves, and upon their place in the mental pattern or complex under investigation. This dependence is brought out, *e. g.*, in the familiar psychophysical experiments that investigate the influence of varying quality or intensity

upon the absolute difference-limen with maximal concentration of attention. The most suitable material is again offered by space-perception, especially by the difference-limen for estimation of distances. The uniform proportionality which has been found to exist between given distances and the increments necessary to arouse a judgment of just noticeable difference may be most simply interpreted, according to Wirth, in the following way: "the clearness of every single 'element' involved in the comparison of two extensions and contributing to the consciousness of a total distance, is decreased in direct proportion to (the clearness of ?) the total magnitude. The maximal degree of consciousness necessary for a certain and correct recognition of an objective difference can therefore be obtained only by corresponding enlargement of the absolute difference" (p. 204).[1] The influence of independent mental processes upon the clearness of other independent processes has been shown in the experiments of Sante de Sanctis and of Heymans, and in various investigations of distraction of attention. It has also been studied quantitatively by Benussi, who utilized the errors of comparison in the geometrical-optical Zöllner illusions, with variously colored main and secondary lines.

This mutual influence is removed, if the competing mental processes occur successively at such intervals as by trial are found to be most favorable for its elimination. Furthermore, the mutual disturbance of co-exciting mental processes is sometimes more or less counteracted by the fact that the presence of a rival acts as a sort of spur for attention to the primary stimulus. Finally, it must be noticed in this place that a simultaneous execution of two disparate activities sometimes leads to a change of the one of them from its habitual to a newly-elaborated performance; the recitation of poetry, which by itself takes places as a rule in acoustic-motor terms, may be mainly carried on, if performed together with mental arithmetic, in visual-motor terms.

In the following subdivision of the Second Part the author takes up "The Course of Continuous Conscious Events and the Time-Perception."

The mutual interrelation of simultaneous mental processes is represented, in its most concrete form, not in the momentary cross-section of consciousness, but in the longitudinal division which permits the study of a given mental content during a certain interval of time. The necessary conditions for analysis are, aside from the frequent repetition of the identical complex, voluntary apperceptive activity and the continuity of the content. They are realized, *e. g.*, in the experimental studies of mental work, begun by Kraepelin for the adult consciousness and by Burgerstein for the child's mind. Even the interference from such factors as fatigue, distraction, dislike, etc., serves only to make the general conditions more concrete, that is, more like those met with in daily life. On the other hand, it will ordinarily be necessary to eliminate the observer's time-sense, as an indirect motive to his apperceptive activity, by requiring him to abstract from the idea of time, and by objective control and variation

[1] Sie (eine ziemlich gute Proportionalität des absoluten, eben merklichen Längenunterschiedes zu der Gesamtstrecke) scheint aber nun am einfachsten so gedeutet werden zu können, dass die Klarheit jedes einzelnen an der Vergleichsrelation beteiligten "Elementes" der aufeinander bezogenen Extensionen, das zum Bewusstsein einer Gesamtstrecke beiträgt, direkt proportional zur Gesamtquantität herabgesetzt wird. Der maximale Bewusstseinsgrad, welcher zur sicheren und korrekten Erkennung des objektiven Unterschiedes notwendig ist, kann daher erst einem entsprechend vergrösserten absoluten Unterschiede zukommen."

of the temporal conditions. The quantity and quality of the mental work performed under such conditions are a direct and precise measure of the existing degree of consciousness; a measure which, if necessary, may be refined by use of the error-methods.

When this method is applied to the continuous observation of liminal stimuli, it leads to the well-known phenomenon of fluctuation of attention. In reviewing the literature of this problem, Wirth arrives at the conclusion that the fluctuation of liminal auditory sensations is certainly not due to peripheral adaptation. The latter is, however, much more important for vision. Nevertheless, the great irregularities in visual fluctuations, their frequent coincidence with circulatory and respiratory oscillations, and their similarity to fluctuations of supraliminal intensities, are strong arguments in favor of a central seat of the fluctuation.

The study of the Time-Perception itself contributes to the solution of the general problem of the experimental analysis of mental phenomena two important points. The first concerns the development of the individual temporal idea. We have here the factors (*a*) of temporal position or *Zeitlage*, best represented in consciousness by the briefest kind of impression, (*b*) of duration, (*c*) of articulated succession, and (*d*) of empty intervals or pauses. All of these may occur with the most varying degrees of consciousness, and in this respect they are analogous to the factors involved in space-perception. The methods of measuring their degrees involve comparison of two successive time-intervals, the one of which as a rule must be constant, while the duration of the other may vary. It is impossible to enter here upon a detailed review of Wirth's discussion of the various investigations of the time-sense, and of such related phenomena as rhythm, continuous change of sensation, the question of "sensed time," temporal or stroboscopical illusions of moving and resting objects, and the subjective division of time-intervals into equal temporal distances.

The second contribution to be mentioned is the result of the complication-experiments originally derived from the astronomical "eye and ear method." A precise and exhaustive analysis of the factors determining the limen of temporal coincidence and displacement and the magnitude of the errors involved can be secured only by the application of the method of minimal changes, without knowledge on the part of the observer. The liminal values thus found, under varying experimental conditions, are on the whole in agreement. Greater importance, however, attaches to the magnitude and direction of the error resulting from the variation of conditions. With a practically uniform distribution of attention over the two coincident mental processes, the error of displacement is small and as often positive as negative. The same result is obtained, still more clearly, by systematic and skillful practice and general mental alertness. These correcting influences are counteracted by such factors as the regular repetition of the experiments, which demands a more or less rhythmical *Einstellung*, the direction of the moving hand on the dial of the complication clock, and the introduction of more than two simultaneous, similar or disparate stimuli. The explanation of the fact of displacement was first given by Wundt, and Wirth merely restates it in less clear language.

The Third Part, finally, discusses the experimental methods of expression, used especially in the investigation of the affective and volitional aspects of mental life. There are mainly three of these. The first is termed the symptomatic method, and deals with those involuntary innervations and secretory processes of the body which

indicate or accompany emotional and volitional states of consciousness. The symptomatic factors expressing, *e. g.*, excitement and depression, strain and relaxation, pleasantness and unpleasantness, arise out of complex mental contents which for the moment occupy mostly the vague background of consciousness, while the impulsive factors of a voluntary action belong to those clear contents which form the sharp, tapering parts of a clearness-relievo. The interrelations of these two kinds of factors are very intricate, and are seemingly complicated by the facts of sympathetic co-excitation, or of competitive inhibition of such bodily functions as respiration, circulation, etc. In so far as the co-ordination between the mental states and these physiological factors is established, the observations and measurements of the latter allow us to draw inferences from them concerning the degree of consciousness of the affective and volitional factors whose expressions or accompaniments they are.

The second method, especially suitable to the analysis of the volitional aspect of mental life, is the ergographic. Like the former, it has a physiological origin and parallel, in the experiments on the tetanised muscle-preparation which naturally suggested experiments on the living muscle, and developed for that purpose a long list of such apparatus as ergographs, dynamographs, dynamometers, etc. One great advantage of this over the previous method is the direct introspective control of the voluntary effort exerted under given experimental conditions. Corresponding to the co-ordinated physiological factors entering into the results of the previous method, the ergographic method in its turn has to consider the factors of fatigue and practice as well as those of co-fatigue and co-practice of the muscular system involved, and their relation to the volitional factors. The work performed by certain muscles of the body, under a constant voluntary impulse, is measured by the instruments employed and indicates the degree of consciousness of the effort exerted. These results may be affected by other simultaneous mental processes as well as by simultaneous work of a secondary kind. The co-ordination of mental and muscular work is of especial importance for the analysis of the volitional aspect of consciousness.

The third method, finally, is based upon experiments with reactions in the narrower sense. Wirth's general discussion of the components of consciousness in simple and choice reactions, and of their systematic control, does not present anything novel or strikingly different from Wundt's exposition in the 5th ed. of the *Grundzüge der physiol. Psychologie III.* In the last section of the book, however, Wirth takes up the reaction-experiments conducted by himself and Kästner, in which "the clearness of the stimulus motive or the vividness of the presentation of the selected impulse" is measured by the quotient of reaction times obtained under complex and under simple conditions. But a comparison of the results of the simple reactions with the corresponding results of the *Schwellenmethode* shows that an approximate similarity obtains only during an intermediate stage of practice in reaction, while after greater practice (of about five months) the reaction times with different distributions of attention no longer portray any typical distribution of degrees of consciousness in the general clearness-relievo. This incongruence, Wirth thinks, may be explained by assuming that the time-differences in the reactions are not due to gradual change from minimal to maximal clearness of the perceived stimulus as such, but result from the clearness with which a given stimulus *is apprehended as a motive to reaction.* The hypothesis seems to be supported by the results of the choice or 'disjunctive' reactions, in which the one hand was co-

ordinated with the part of the visual field attended to, while the other reacted to changes in the part not attended to. The retarded reaction times for the latter were due to the delayed co-ordination of the stimulus as a motive to the correct hand.

This account must suffice to serve as an indication of the main contents and chief points of interest of a book which is as rich in detail as it is broad in scope. The interweaving of observation and generalization would only increase the value of the work, if the author's style were less involved and less abstract. As it is, the combination greatly enchances the difficulty of understanding a work whose language is often hopelessly confusing. The author sometimes repeats himself unnecessarily, thus loading his sentences with cross-references and obscuring their grammatical construction; sometimes he coins terms, to be employed in a very technical sense, without expressly defining them; and sometimes he uses familiar words in a very specific connotation without further explanation. For these reasons, the reviewer finds it difficult to enter upon a detailed criticism, as he is unable to judge in how far he has correctly understood and interpreted the work. He disagrees, in general, with the author's definition of degree of consciousness and his method of measuring it, with his treatment of the fluctuations of attention, with his discussion of the nature of apperceptive and attentional activity, and with the general presentation of the emotional and volitional aspects of mental life. The first two points of difference concern matters of fact, which have to be decided by experimental method; the two last are still too closely bound up with theory and hypothesis to allow of the hope of satisfactory solution in the near future. The reader who, for the time being, accepts Professor Wirth's point of view will find the author's presentation and interpretation of his subject-matter thoroughly systematic and consistent. While his standpoint is essentially Wundtian, he has also incorporated various Lippsian elements and even uses terms which remind one of the Austrian school. His knowledge of the history and literature of the science makes his treatment of its experimental development especially valuable, and his work will therefore be appreciated by many as a source-book of psychological methods and references.

PSYCHOLOGICAL LITERATURE

An Arraignment of the Theories of Mimicry and Warning Colors. By ABBOTT H. THAYER. Reprinted from the *Popular Science Monthly*, December, 1909. pp. 550-570.

Concealing Coloration in the Animal Kingdom: an Exposition of the Laws of Disguise Through Color and Pattern. Being a Summary of Abbott H. Thayer's Discoveries. By GERALD H. THAYER. With an Introductory Essay by A. H. Thayer. Illustrated by Abbott H. Thayer, Gerald H. Thayer, Richard S. Meryman and others, and with photographs. New York, The Macmillan Co., 1909, pp. xix, 260. Price $7.00 net.

Modern biology has occupied itself, in great detail, with the problems of organic coloration. The main field of discussion has naturally been that of the surface colors of animals; for internal and invisible coloration can have no biological significance, and "the dominant coloring of plants is an essential element in the paramount physiological activity of chlorophyll" (Poulton). In general it is held that the visible colors of animals, and in exceptional instances those of plants, have, for immense periods of time, been modified and arranged to assist in the struggle with other organisms or in courtship. There are, first of all, the phenomena of protective and aggressive resemblance, colors which enable an animal to conceal itself from its enemies or to approach its prey unseen, procryptic and anticryptic colors. There are warning colors and recognition marks, aposematic and episematic characters. There is the great mass of observed facts grouped under the head of mimicry: Batesian or pseudaposematic mimicry, an advantageous or deceptive resemblance borne by palatable or harmless species (the mimics) to others that are unpalatable or otherwise specially defended (the models); and Müllerian or synaposematic mimicry, the advantageous adoption of a common advertisement by specially defended species, whereby the loss of life incurred during the education of inexperienced enemies is contributed jointly by the similar forms instead of by every species independently. And, finally, there are the epigamic characters, explained by Darwin's theory of sexual selection. Color, at first incidental or non-significant, is thus supposed to have furnished admirable material for selection, natural and sexual, and to have played a large part in the mechanics of organic evolution.

With the central doctrine, that surface color is of high importance in the struggle for life, the author and inspirer of the two works under review has no quarrel. On the contrary, he very strongly insists on the adaptive character of coloration, of color itself and of color-pattern. He assumes the truth of natural selection; he believes, with Darwin, that natural selection operates upon small chance variations; he emphasizes the value to a species of adaptive preparation for such circumstances in its life as are of only occasional occurrence. But he insists, just as strongly, that expert judgment in the matter of the importance of coloration lies, not with the biologist, not even with the field-biologist, but with the student of color, the artist and the psychologist. Biologists have, with a few salient exceptions, appraised the conspicuousness or inconspicuousness of color and pattern from

the isolated specimen and, what is equally disastrous, from the human point of view. The right method is to set the animal in its natural surroundings, to bring its coloration and movement into relation with the colors and changes of its background; and then, having done this, to contemplate the animal from the point of view not of man but of those other animals which, whether as enemies or victims, have an interest in its discovery. If these rules are followed—and they surely need no defence—we have a surprisingly simple and uniform result: all creatures that ever prey or are preyed on prove to be colored in the manner best calculated to conceal them at the moments of their greatest need.

At first reading, this rule seems far too simple to be true; and as one reflects upon it, a large number of what seem to be obvious exceptions present themselves to the mind. Mr. Thayer has, therefore, done well to take the line followed in his book; to give his readers a great array of ocular demonstrations, clear of theoretical discussion. But he has also done well to preface the book by the article in the *Popular Science Monthly*, and to formulate his negations before he offers the positive testimony for his position. The article is intended to demonstrate the fallacy of the badge and warning color theories; and though it is not, in the same sense, an attack upon mimicry, it inevitably suggests that this theory cannot survive the demise of the others. Like the book, it has for its backbone a series of plates that demonstrate, beyond dispute, the primal effect of patterns and the wrongness of the older hypotheses about them. It begins by pointing out how "Darwin's erroneous supposition that a conspicuous mark on an object makes the object itself conspicuous has been built on and rebuilt on by the leaders of zoölogical research, even down to the present day. Entomologists, especially, make much of the supposed power of sharp and strong patterns to render conspicuous that particular part of the insect which they occupy." A fairly elementary consideration of the laws of irradiation, contrast, induction, as they operate under the conditions found in nature, is enough to prove that the effect of these patterns is the very opposite. Save for the comparatively rare moments when monotone exactly matches its background, pattern in aerial animals works for concealment in direct ratio to its own conspicuousness and elaboration.

The limits of space forbid my following the writer into his secondary arguments against warning colors and recognition marks; the fundamental arguments are, as I have said, furnished by the plates themselves. It must suffice to say that they are concerned in part with the intrinsic improbability or incoherence of the underlying theories, and in part with alternative explanations based upon the laws of visual perception mentioned above. The general argument against mimicry runs as follows: "As a few hours' experimenting in obliteration by juxtaposition of patterns will prove to any student, the optical laws which govern it are so absolute that one is not surprised to find that the whole world's butterflies have scarcely three different schemes of pattern. The principle of pattern arrangement in the famous 'mimetic' groups is out and away the predominant one over the whole globe. If this is the case, is it strange that in each most swarmingly populated seat of butterfly life there prove to be a number of species which, living in the very same station, and with seemingly identical habits, have, in obedience to this great pattern-law, practically identical patterns and form? We see in the ocean, for instance, even mammals wearing the shape and color of fishes." If this argument is sound—and Mr Thayer states that he has found no exceptions at all, after seven years of almost continuous investigation of the pat-

terns of the world's butterflies—Professor Poulton's occupation is gone. It is a little curious that Poulton himself, while he recognizes the principle of counter-shading and has even discovered its dynamic operation in the Chamæleon, apparently fails to see the danger which threatens his favorite studies. Yet Mr. Thayer's law, if it be established, makes the tracing of mimetic resemblances at best a mere side-issue, and at worst a futility.

It is to the establishment of the law that the book before us is devoted,—surely, for those who are interested in general biology, one of the most interesting books of all the recent crop. One naturally turns at first to the colored plates: to the Peacock amid foliage, the Ruffed Grouse in the forest, the Cottontail among ferns and moss and grasses. It is unnecessary to say that the creatures are admirably pictured, but it is pleasant to be able to add that the mechanical reproduction is satisfactory. These illustrations are convincing, as regards their special subjects; but their effectiveness is increased by another group of plates, in which the argument that they embody is generalized,—the plates showing drakes of the Wood Duck in the water, Blue Jays against snow, Birds of Paradise in the tropical forest, caterpillars of various species on leaves and twigs; and in particular, the plates which place a line of Spoonbills or Flamingoes against the dawn or sunset sky. These plates are nothing less than illuminating; and the Publishers' Announcement is keeping to the literal truth in its statement that "the colored plates anticipate the future of wild animal illustration, showing, for the first time, concealing colors *in operation*." The picture of the Copperhead Snake caps the climax; the animal lies on a bed of dead leaves, and the plate is faced by a blank page from which its outline has been cut. Seen on the white ground, the animal is obvious, of course, by form and color; seen on the background of leaves, it disappears, and even a close scrutiny can trace its contour only with difficulty and by repeated reference to the blank form. All this must be seen to be believed.

There are sixteen colored plates, and there are a hundred and forty figures in black and white,—models, diagrams, photographs from nature. Here is the array of ocular demonstrations, upon which the thesis of the book mainly rests. That thesis is, to repeat, that "the entire matter [of animal coloration] has been in the hands of the wrong custodians. . . . It properly belongs to the realm of *pictorial art*, and can be interpreted only by painters. For it deals wholly in optical illusion, and this is the very gist of a painter's life. . . . This book demonstrates that the colors, patterns, and appendages of animals are the most perfect imaginable effacers under the very circumstances wherein such effacement would most serve the wearer, . . . the most gorgeous costumes being, in their own way, climaxes of obliterative coloration scarcely surpassed even by moths or inchworms. . . The means of objects' recognizability, no matter how they are colored or marked, is almost always their *silhouette—i. e.*, their outlines in 'relieving' darker or lighter or differently colored against their background. . . Patterns on animals' coats are the utmost that Nature can do in opposition to these potent vicissitudes of silhouetting." As regards terminology, Mr. Thayer recognizes Obliterative Coloration and Mimicry as the two main principles of Protective Coloration. The former principle covers all those concealing colors which serve to render the animals invisible in their native haunts. It is mainly based on counter-shading,—a discovery which was originally published in *The Auk* in 1896, has often been discussed and illustrated in recent years, and may therefore be assumed to be familiar to the reader. The second principle covers all those colors which

make an animal look like something else than what it really is; mimicry, that is, aims at deceptive visibility, while obliterative coloration aims at invisibility. It is, perhaps, regrettable that the writer uses mimicry in this wide sense, now that its use has been definitely restricted in current biological discussion; obliterative coloration might better be paired with, say, obliterative resemblance. However that may be, mimicry plays but a small part in the work; only a few examples are given, and these chiefly from among the lower orders; in the higher orders it has, in Mr. Thayer's opinion, a very secondary importance.

After the introductions—general by Mr. A. H. Thayer and special by Mr. G. H. Thayer—comes a series of seventeen chapters, dealing mainly with the markings of birds; three chapters are then devoted to mammals, one to fishes, one to reptiles and amphibians, and three to insects and spiders. It is difficult, where so much is given, to make a selection. But, if a choice is to be made, we may perhaps begin with the skunk, to which a good deal of attention is paid both in the *Popular Science Monthly* article and in the book. Belt, in *The Naturalist in Nicaragua*, tells us that at night "the skunk goes leisurely along, holding up his white tail as a danger-flag for none to come within range of his nauseous artillery." He describes the large white tail, laid over against the black and white body, as producing a very conspicuous effect in the dusk. There could not be, according to Mr. Thayer, a better instance of the naturalist's fallacy; the skunk's markings are beautifully obliterative. First, the mixed black and white of the body cause the animal to fade from sight at a short distance, and may even, at a near view, confuse themselves with details of forest and shrubbery. Secondly, a night is rarely so dark that a solid form within a foot of the eye would not show dark against the sky or against the light parts of the forest ceiling; hence, the white forehead markings, borne by the skunks and other grubbers of small surface life; the white markings counterfeit the sky, and are thus, as it were, seen through, as if transparent. Thirdly, the animal's white top has a cardinal function to perform; it effaces his upper contour against the sky to inhabitants of the turf; it too, therefore, is functionally transparent, an open space in the depths. And lastly, the skunk's tail is normally a mixture of black and white hairs, like a gray cloud. These points are clearly shown in Figs. 4 and 8-12 of the *P. S. M.* article, and in Figs. 95-103 of the book. The compound picture of Fig. 103 may, indeed, be said to furnish a key to the whole volume.

For a second instance, we may take the coloration of the Blue Jay, whose entire costume proves to be an arrangement of the brilliant colors of sunlit winter scenery,—his white the sunlit snow, his blues the varying shadows, his black the tree trunks and bare twigs, his ashy sides the haze of winter bushes. A glance at the plate facing p. 107 will train the reader's eye for observation of these facts in the open.

Lastly, a word may be said on nuptial colors. These, Mr. Thayer insists, are not to be ascribed to sexual selection, nor do they serve to render their wearers conspicuous. On the contrary, they represent plainly an increase of such potency of obliterative coloration as belongs to all gorgeously varied costumes, and this at the very moment when concealment is most needed. The puffin's bill, as he stands on guard at the mouth of the nest-burrow, serves by its gaudy color to obliterate the dark mouth of the hole, and at the same time substitutes a semblance of flowers to complete the deception. The moment his domestic duties are over, and he is back in the open sea, we behold

the bird dressed again in the universal ocean-and-rock colors of his habitat.——

Mr. Thayer's indictment of the naturalists may, if I have understood him aright, be summed up in four propositions. They have failed to realize that conspicuousness is solely a matter of relation to background; that black is conspicuous against white, and white against black, but that neither is conspicuous in and for itself. They have forgotten that there may be a vast difference between the conditions under which they see an animal and the conditions under which he is present, in nature, to his enemies or victims; so that his conspicuousness to human eyes may be altogether irrelevant. They have assumed, without test, that patterns make their wearers conspicuous. And, rightly noting the fact that aerial species in general are relatively conspicuous, they have failed to inquire whether any change of costume would remove or lessen this source of danger. In support of his own view—less, perhaps, a view than a discovery—Mr. Thayer offers a multitude of actual observations, covering all the main divisions of the animal kingdom. The labor involved in these observations has been immense, but has evidently been a labor of love. As such, it is its own reward; though external reward will also come in the conversion of many readers to the painter's, or, as I should prefer to say,—if this change of terms may be made without injustice,— to the psychological standpoint. No doubt, there remains the great and ultimate question of causation; but that remains for the followers of Bates and Müller no less than for Mr. Thayer.

E. B. TITCHENER.

Handbuch der Schwachsinnigenfürsorge mit Berücksichtigung des Hilfsschulwesens, Herausgegeben von Hans Bösbauer, Leopold Miklasu. Hans Schiner. Wien, Karl Graeser & Kie, 1909.

The following are the chapter headings: I. The nature ef Feeble-mindedness. II. The kinds of Feeble-mindedness. III. Symptoms of feeble-mindedness (a) Bodily (b) Psychic. IV. Causes. V. Measures for prevention. VI. History of the movement for care of the feeble-minded. VII. Education and treatment of the feeble-minded. VIII. Forms of education. IX. Organization of institutions and "Special classes" (Hilfsschulen). X. Instruction. XI. Personality of the teachers. XII. After-care for the children who go out from the institutions and classes. XIII. Legal protection for, and military service by, the feeble-minded. XIV. Bibliography.

As is evident from the contents, the subject is discussed in many phases. The writers have gone over the literature with great thoroughness and have brought together many important facts and opinions. The book is conservatively written and gives no new data nor takes any advanced stand on any of the important questions involved. Most of the opinions quoted are given without criticism. The usual "causes" are given and discussed without much consideration of their relative importance. *E. g.*, alcohol is taken as a serious matter and important cause. No hint is given that any one doubts that it is an important cause.

In discussing "prevention," most everything is spoken of except the regulation of marriage—which would probably stop 80% of the trouble. Mention is made in fine print of a "*strange kind*" of law that has been passed in Indiana (the law authorizing castration). That is the only reference to that important matter. In discussing symptoms and education and treatment, the authors labor under the difficulty of too few heads of classification. "Feeble-mindedness" covers a wide range of conditions. It is not possible to discuss the

symptoms of feeble-mindedness in any helpful way without specifying many degrees of defect. The same is true of the education and treatment. The authors do make use of the term "idiot" for the lowest grade, but even then there is too much variation in what is left.

But this is a difficulty that we all labor under more or less. Three groups are fairly well marked out, but beyond that there is not much unanimity in the classification.

The book is well written and is a great contribution to our literature on the subject.

The bibliography is very extensive and a valuable adjunct. Its value would have been still greater if it had been classified or supplied with a subject index. The book ought to be translated into English. Our literature, that is available for any but the specialist, is far too scanty.

HENRY H. GODDARD.

The Survival of Man; a Study in Unrecognized Human Faculty. By Sir OLIVER LODGE, F. R. S. New York, Moffat, Yard & Co., 1909. pp. viii, 361. Price, $2.50 net.

In this volume, Sir Oliver Lodge illustrates, by reference to investigations pursued during the past quarter of a century, the manner in which his own conviction of man's survival of bodily death has been acquired, and the kind of evidence by which he believes that this conviction will in due course be scientifically justified. The investigations which he reports deal with the phenomena of experimental telepathy, of spontaneous telepathy and clairvoyance, and of automatism and lucidity; an account of his experiences with the controverted and often discredited 'physical phenomena' associated with exceptional mental states is, the Preface states, reserved for another volume.

The book is frankly popular in its appeal, and contains nothing that is new to a student of the subject. Thus, the Introduction reprints the author's address delivered as president of the Society for Psychical Research in 1903; the evidence for telepathy is reprinted from the *Journal* and the *Proceedings* of the same Society, or from such well-known sources as Myers' *Human Personality*, and the *Phantasms of the Living:* and so on throughout. The writer's reliance is on the cumulative character of the testimony. No doubt, there are many readers who will be impressed by this feature; but there will be others, of stiffer mental fibre, who will insist that a chain is no stronger than its weakest link, and that, as every one of the links here passed in review has its weak place, the assemblage has no great claim to scientific consideration.

The discussion, as we should expect from the author's general reputation, is within its limits fair and candid. It is a question, however, whether the limits themselves should not, in the interests of science and truth, have been extended. Sir Oliver Lodge might, for instance, have inserted a chapter on the range of the unconscious whisper, on the indicativeness of unconscious head-movements, on muscle-reading, and on the various forms of experiment upon normal suggestibility published in the various psychological journals. The facts and conclusions of such a chapter would, surely, not have been out of place; it is a canon of scientific procedure to furnish negative as well as positive instances. The author is probably unaware of much of the work done upon these topics, in recent years, by experimental psychology. At the same time, its inclusion would have rendered his book less one-sided than it now appears.

FRANCIS JONES.

The Right to Believe. By ELEANOR HARRIS ROWLAND. Boston and New York, Houghton Mifflin Co., 1909. pp. xv, 202. Price $1.25 net.

The writer of this little book, who is an instructor in philosophy and psychology at Mt. Holyoke College, explains its purpose and origin as follows. "I have happened to engage in discussion with some twenty persons, whose minds were more or less at sea in religious matters; and while their starting-points were different, their difficulties fell into somewhat well-defined types. The method which I suggested for answering certain of these questions was an apparent help in enough cases to justify me in thinking it could stand the test of a wider audience. To any criticism of the book (and there may be many) I can always reply: It has answered the questions of a dozen people. I claim for it nothing more." The book itself falls into six chapters, which discuss in a vivid and interesting way, though in every case with a foregone promise of orthodox issue, the necessity for a belief, the existence of God, the nature of God and of man, the divinity of Christ, the problem of evil, and prayer. Miss Rowland makes brilliant use of her philosophical and psychological knowledge; she meets the 'plain man' on his own ground of common sense logic, and beats down his scruples and objections with weapons like his own; her special pleading is concealed, partly by rhetorical skill, partly by intensity of personal conviction. She has, if the reviewer is not mistaken, a real gift of exposition in popular philosophy; and her greatest danger, if he reads her work aright, is facility. W. JENKINS.

Spinoza's Short Treatise on God, Man and Human Welfare. Translated from the Dutch by L. G. Robinson. Chicago, Open Court Publishing Co., 1909. pp. xxiv, 178. Price, cloth, $1.25 net; paper, 50c.

Students of philosophy are under a very real obligation to Miss Robinson for this translation of the Short Treatise—the first English version of Spinoza's first philosophical work. The translation has been made from the Dutch text of Van Vloten and Land, with frequent reference to the German of Schaarschmidt. It is, perhaps, rather over-literal; but in general, so far as the reviewer can judge, it is accurate and dependable. Blemishes of style and possible slips in rendering can, at all events, be corrected by the instructor in the classroom and by the translator in subsequent editions. Whether it was worth while to reprint, as introduction, Schwegler's chapter on Spinoza is, perhaps, questionable. Our dominant feeling should, however, be one of sincere gratitude to translator and publisher for making the Short Treatise accessible to our students.

JAMES FIELD.

Die Mechanik des Geisteslebens von MAX VERWORN. Teubner, Leipzig, 1910, 2te Aufl. pp. v, 114.

The first edition of these lectures upon fundamental problems of physiological psychology, by one of the foremost of German physiologists, has already received notice in this *Journal* (XIX, 1908, 266 f.). The second edition differs from the first only in a slight expansion of the text at certain points and some increase in the number of illustrations. E. C. S.

BOOK NOTES

Könstlivet i Opdragelsen. P. F. LANGE. 2. Oplag, Aarhus, 1909. pp. 78.

This little work on "The Sexual Life in Education," is by one who has had some seven years' experience as a teacher in Denmark. The author, now a school-inspector, has been professor in Sorö College and also Vice-President of Marselisborg College. Professor Lange's chief conclusions are as follows:

1. Lying and fabrication are absolutely evil here. Children ought to have open and truthful information from their parents, as soon as their questions are asked out of real interest. 2. At school the text-books used by the children in natural history ought to give the necessary data concerning sexual conditions. 3. Co-education of boys and girls is a hindrance to really intelligent treatment of this question. 4. There exists no proof whatever that the healthy child can incur serious injury to his physical or his mental health through occasional masturbation. 5. There is abundant, irrefutable proof that children, who have been frightened by exaggerated and untruthful stories of the evil effects of masturbation have from the fear induced suffered greatly both in bodily and in mental health. 6. The most dangerous and most injurious effects are observed on those occasions when the child is suffering simultaneously from sexual and religious afflictions. 7. The chief task of parents is preventive. It consists principally in giving the child reasonable physical and mental instruction. If there is direct interference, it must be with caution and gentleness. 8. It is most important that the child should not make to others, and particularly not to himself, promises which he cannot keep. 9. The school has, in relation to the question of masturbation, no direct task. 10. When boys go out into practical life, they ought to be given, either on the part of the home or on that of the school, full information as to the ease with which sexual disease may be acquired and the great dangers therein both to themselves and to others. 11. First and last, meanwhile, come the relations of trust and confidence between parents and children. Not glittering, waste moral preachings, but life lived together with children and for them. 12. A natural consequence of the commandment, "Thou shalt honor thy father and thy mother," is, "Thou shalt honor thy children."

A. F. C.

The Sexual Life of Our Time: its Relation to Modern Civilization. By IWAN BLOCH. Translated from the 6th German editon by M. Eden Paul. Rebman, Ltd., London, 1909. pp. 790.

The author's aim in writing this book was to compile a pretty complete encyclopedia on the sexual sciences. It certainly is the most comprehensive work on the subject which is extant in English, treating nearly all the various aspects of it and with remarkable plainness and detail. The author does not address himself to special topics and is therefore less exhaustive upon these than writers like Havelock Ellis.

Pubertät und Schule, von A. CRAMER. B. G. Teubner, Leipzig und Berlin, 1910. 16 p. (Schriften des deutschen Ausschusses für den mathematischen und naturwissenschaftlichen Unterricht. Heft 4.)

The chief thesis of this work is that the school does not exist to benefit psychopathic individuals, but that teachers of the future must be more or less acquainted with the clinic of puberty, in order to understand their children in general during their most critical period of life, and in order to distinguish between what is normal and what is abnormal in them.

Studies in the Psychology of Sex, by HAVELOCK ELLIS. Volume 6, Sex in Relation to Society. Davis, Philadelphia, 1910. 656 p.

In his previous five volumes of studies, the author has dealt with the sex impulse mainly in relation to its object, leaving out of account the external persons and the environmental influences. In this volume he now considers its relations to society and discusses in a more summary way the manifold, important problems that are presented. The previous work entered a more or less neglected field, which required analytic care and precision; but the ground covered by this volume is more worked. It treats of the mother and her child, sexual education, nakedness, evaluation of sex love, the function of chastity, the problem of abstinence, prostitution, the conquest of venereal diseases, sexual morality, marriage, the art of love and the science of procreation.

Das sexuelle Problem und seine moderne Krise, von E. MERTENS. M. Kupferschmid, München, 1910. 474 p.

The author first treats of propagation in animals and plants, of the human organs, of pre-marital hygiene and regimen for boys and girls, the ethnology and early history of marriage, sexual hygiene within it, prostitution, diseases, abnormalities, stages of development, with final sections on the right of motherhood, free love, flirtation, emancipation of women and sexual education.

The Sexual Life of Woman, by E. HEINRICH KISCH. Authorized translation into English by M. Eden Paul. Rebman Company, New York, 1910. 686 p.

The writer treats first the sexual epoch of the menarche—its retardation, acceleration and its pathology generally, the causes of the determination of sex, fertility, sterility, various abnormalities, the age of senescence—its normality and pathology. The work is enriched by ninety-seven cuts.

Die Syphilis der Unschuldigen (Syphilis Insontium), von OSKAR SCHEUER. Berlin, Urban & Schwarzenberg, 1910. 239 p.

Although the author is a professor of sex and skin diseases, his book has a certain pathos for it is devoted largely to a description of cases where syphilis has been pandemic, that is, epidemic or endemic, and where it has been sporadic in various senses, and a description of very many ways by which in the modern clinic it is found to be imparted to the innocent.

Die Psychoneurosen und ihre seelische Behandlung, von PAUL DUBOIS. Uebersetzt von Dr. med. Ringier in Kirchdorf bei Bern. Vorrede von Dr. Déjerine. Zweite durchgesehene Auflage. Bern, A. Franche, 1910. pp. 484.

This book consists of a course of five and thirty lectures delivered in the University of Bern. The author is well known as being not

only a physician, but a psychologist and deserves to be reckoned as one of the pioneers of the new psychic treatment of disease. Some of the cases herein described are of great interest and, indeed, have become almost classical in clinical literature. Great stress is laid upon ethical dialectics, upon self-control, upon the doctrine that virtue is knowledge, upon moral insight, discipline, dialectics of pure feeling over against that of pure reason. The author freely grants that there are various forms of neurasthenia that cannot be healed by psychotherapy and upon which suggestion and belief, conviction, persuasion have little power.

Psychische Grenzzustände, von Dr. CARL PELMAN. Zweite durchgesehene Auflage. Friedrich Cohen, Bonn, 1910. pp. 316.

This is an interesting study of borderline or marginal phenomena, dealing with such very diverse themes as criminals, suicides, regicides, Cæsarian illusions, sex abnormalities, drunkards, bums, vagabonds, liars, querulants, victims of passion, pride, jealousy, peculiar people, fools, imperative ideas, hypnotism, illusions, ecstatics, seers and prophetesses, witches and possessed, psychic epidemics, etc.

Grundlagen für das Verständnis krankhafter Seelenzustände beim Kinde, von Dr. med. HERMANN. Hermann Beyer & Söhne, Langensalza, 1910. 180 p.

The author first treats of the brain, of concepts and thoughts and their disturbances; then of feeling, striving, willing, acting and their disturbances. This constitutes the general part. He then considers traits of degeneration in the different faculties, their most frequent types in children, congenital weak-mindedness and acquired disturbances, epilepsy, etc.

Wahnsinnige als Herrscher und Führer der Völker, von WILHELM HENCKEL. Otto Gmelin, München, 1910. 167 p.

These psychiatric historical studies are five in number: the first treats of Peter the third of Russia; the second of Paul the first of Russia; Nebuchadnezzar, the king of Babylon; Saul, king of Israel; and the last of Ludwig second, king of Bavaria.

Abnormal Psychology, by ISADOR H. CORIAT. Moffat, Yard & Co., New York, 1610. pp. 325.

The writer first explores the subconscious, defines it, gives modern theories about it, characterizes its mechanism and how it may become diseased. Then follow chapters on automatic writing and crystal gazing, testing and analyzing the emotions, sleep, dreams, hypnosis and the analysis of mental life with an interesting illustration. Under the diseases of the subconscious, he treats of losses of memory and their restorations, illusions, splitting of personality, hysteria, psychasthenia, neurasthenia, psycho-epileptic attacks. The book is an interesting contribution to the subject that is now greatly engaging the attention of psychologists the world over.

Anti-Pragmatism, by ALBERT SCHINZ. Small, Maynard & Co., Boston, 1909. 317 p.

This is a translation, with some modifications, of the author's book on the same subject in French. It is, on the whole, the ablest and most convincing criticism of pragmatism to be found in English. In an appendix the author answers some of the criticisms directed against the French edition of his work.

The Principles of Pragmatism, by H. HEATH BAWDEN. Houghton, Mifflin Co., Boston, 1910. 364 p.

The author here brings together many interesting essays of his which have appeared in various places before, and now gives them unity. He does not aim to construct a system but to show how in pragmatism we may establish the basal conceptions of a new philosophy of experience; hence his seven chapters are headed: Philosophy; Experience; Consciousness; Feeling, thinking, truth; Reality; Evolution and the absolute; Mind and matter.

The psychology of reasoning, by W. B. PILLSBURY. D. Appleton & Co., New York, 1910. pp. 306.

This volume is based on eight lectures delivered at Columbia University, in 1909. It attempts a brief statement of the place of logical processes, particularly judgment and inference in the concrete individual consciousness, not including the social aspects of reasoning, but its results in action for which the author refers to Baldwin and Dewey respectively. The chapters are the place of reasoning in psychology, belief, meaning and the concept, judgment, judgment and language, inference, proof and the syllogism, the nature of inductive proof, degrees of truth, modality and probability.

The influence of Darwin on philosophy and other essays in contemporary thought, by JOHN DEWEY. Henry Holt & Co., New York, 1910. pp. 304.

Besides the introductory lecture of 19 pages which gives its title to the book, the following articles are here printed, or reprinted from other publications: Nature and its Code, a conversation; Intelligence and Morals; the Experimental Theory of Knowledge; the Intellectual Criterion for Truth; a Short Catechism Concerning Truth; Beliefs and Existence; Experience and Objective Idealism; the Postulate of Empiricism; "Consciousness" and Experience; the Significance of the Problem of Knowledge.

Das Wesen des menschlichen Vorstandes und Bewusstseins: nach monistischer und dualistischer Auffassung. Von ALBRECHT RAU. Ernst Reinhardt, München, 1910. pp. 236.

This sums up this very ingenious biologist's theories upon the subject of the nature of consciousness with reference to monism and dualism. It is hard to state concisely his general theory. He holds that the torch of knowledge never goes out so long as there is any preserver or bearer for it and that it is the highest function of the state to provide and to keep investigation absolutely free. Again, all pure psychic reciprocities take place within the single soul and all intercourse of souls one with another is only indirect and mediated in physical ways. What we want is a test of reason by experience and history and to do this is the philosophic problem of the present day. Sense knowledge is the typical form of all kinds of knowledge.

Stoic and Epicurean, by R. D. HICKS. Charles Scribner's Sons, New York, 1910. pp. 412. (Epochs of Philosophy.)

It was a happy thought of this writer to bring the systems of Zeno and Epicurus together to show their fundamental similarity. Both exalted practice above theory and gave to sense and experience their full weight. Both were crude forms of realism such as the temper of the age in its reactions from extreme intellectualism favored. The success of the schools, too, was important, reaching its height among the practical Romans. The author has studied the original authors as well as the various recent writers who have gone into the field.

He treats of the earlier Stoics and pantheism; Stoic philosophy and epistemology; Moral idealism; the Teaching of the later Stoics; Epicurus and hedonism; the Atomic theory; the Epicurean theology; Scepticism in the academy; Carneades; Eclecticism; Ænesidemus and the revival of Pyrrhonism.

The philosophy of enlightenment, by JOHN GRIER HIBBEN. Charles Scribner's Sons, New York, 1910. pp. 311. (Epochs of Philosophy.)

After characterizing the age of enlightenment in general, the author begins the serious work of his book by considering Locke's inner and outer world; Berkeley's idealism; Hume's scepticism; the Materialistic movement in England; Rousseau's philosophy of feeling; the Philosophy of Leibnitz; the Conflict of typical philosophical influences in Germany; the Critical philosophy of Kant; the Practical influences of enlightment.

The Place of Animals in Human Thought, by The Countess EVELYN MARTINENGO CESARESCO. T. Fisher Unwin, London, 1909. 376 p.

This book owes its origin to the question "whether the psychology of animals has not equally some relation to the science of religions?" —a problem raised by Count Goblet d'Alviella at the Oxford Congress in 1908. The topics treated are: Soul-wandering as it concerns animals, the Greek conception of animals, Animals at Rome, Plutarch the humane, Man and his brother, the Faith of Iran, Zoroastrian zoölogy, a Religion of Ruth, Lines from the Adi Granth, the Hebrew conception of animals, "A people like unto you," the Friend of the creature, Versipelles, the Horse as hero, Animals in Eastern fiction, the Growth of modern ideas about animals. The book is copiously illustrated with photographs from ancient monuments and other animal monuments.

Physiology of the Special Senses, by M. GREENWOOD, JR. Longmans, Green & Co., New York, n. d. 239 p.

This is designed for two classes of readers: students of psychology who wish to know more about the physiological side, who are advised to read in connection C. S. Myers's text-book of experimental psychology, and, second, to the physiologist. The book seeks to fill the gap between the ordinary text-book of psychology and original memoires. The topics treated are: Fechner's law; Cutaneous sensations generally; Pain; Protopathic and epicutic sensibility; Taste and smell; the Sense of position and movement; Hearing, with historical sketch; the Ear; Comparative anatomy of vision; Retinal processes; Visual adaptation; Theories, trichromatic vision; Dichromatic vision; Historical theories; the Young-Helmholtz theory and hearing; Contrast space. There are 28 figures in the text.

The hygiene of the soul, a memoir of a physician and philosopher. By GUSTAV POLLAK. Dodd, Mead & Co., New York, 1910. pp. 209.

This is a memoir of Feuchtersleben with special reference to his dietetics of the soul first published in 1838 and translated into English by Ouvry in 1852. Feuchtersleben was born in 1806 and died in 1849. Although his chief interest was medicine, he studied philosophy, literature and was a poet of note. The revived interest in psychotherapy makes this work a timely one.

Die mnemischen Empfindungen in ihren Beziehungen zu den Originalempfindungen, von RICHARD SEMON. Erste Fortsetzung der "Mneme." Wilhelm Engelmann, Leipzig, 1909. pp. 392.

This work is in partial fulfillment of the promise of the author in his first book entitled "Die Mneme"— the preservation principle in

the change of organic processes. He originally intended in this first continuation of his theme to attend solely to the pathology of the mneme, but has here radically modified his plans. After a discussion of terminology and the limitations of his scheme, he discusses in the first part the original sensations, their synchronous phase, single sensations and their associations, defines the idea of the field of sensation, discussing homophony, the akolunthan phase of original sensations. The second part is devoted to mnemic sensations and describes first the vanishing of the original and the persistence of the engramms. He then discusses the single engramms and their simultaneous complications, the individually acquired treasury of engramms, ecphoria and the various forms of association, components and indiscernibles, the relation of the mnemic to original sensations and their lapse, repetitions and the conditions of mnemic homophony, abstractions, differentials, modifications, comparisons, the engraphic action of homophonous components, contrast of original and mnemic sensations, general results in the sense of simplification and analysis.

Die Insekten in Sage, Sitte und Literatur, von KARL KNOREZ. Annaberg, Sachsen, Grafers Verlag (Richard Liesche), 1910. 151 p.

This is divided into four parts, the bee and honey, the flea and louse, flies and spiders, and all kinds of creepers and fliers.

L'Illusion Paradoxale et le Seuil de Weber, par MARCEL FOUCAULT. Coulet et Fils, Montpellier, 1910. 211 p. (Travaux et Mêmoires de Montpellier. Série Litteraire V.)

The first chapter gives a brief history with preliminary experiments, methods and analysis. The second treats of measurements of the threshold, and the third of its conditions.

Klinische Beitrag zur Frage der Alkoholpsychosen, von WILHELM STÖCKER. Gustav Fischer, Jena, 1910. 289 p.

This work treats of the following classes of cases with the topics, Epilepsy as the basis of chronic alcoholism; Maniacal-depressive insanity; Dementia præcox; Other psychoses and psychopathies; and Obscure cases. Under these five groups, the author brings together descriptions of many cases.

Naturvölker und Kulturvölker. Ein Beitrag zur Socialpsychologie, von ALFRED VIERKANDT. Leipzig, Duncker & Humblot, 1896. 497 p.

This contribution to social psychology is an attempt to contrast the psychic and social life of primitive people with that of civilized and cultured races.

CORRECTION.

In a brief notice of Dr. Otto Lipmann's *Grundriss der Psychologie für Juristen* in the January number of the *Journal*, p. 174, it was inadvertently stated that the fifth chapter of the work dealt with the *Tatbestandsdiagnostik* "from the standpoint of Freud." This statement was unfortunate, as Dr. Lipmann is not at all a pupil of Freud, but on the contrary takes a critical attitude toward "psychoanalysis." Freud and his school make characteristic use of the *Tatbestandsdiagnostik* in seeking to discover psychic complexes *unknown* to the experimenter at the time of the experiment; the workers in legal psychology use it simply to determine whether a complex *known* to the experimenter is also known to the subject of the test.